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SELECTED FROM

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AND FROM

FOREIGN JOURNALS.

EDITED BY

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1841.

"Every translator ought to regard himself as a broker in the great intellectual traffic of the world, and to consider it his business to promote the barter of the produce of mind. For, whatever people may say of the inadequacy of translation, it is, and must ever be, one of the most important and meritorious occupations in the great commerce of the human race."—Goethe, *Kunst und Alterthum.*

PREFACE TO THE SECOND VOLUME.

IN the Advertisement to the Seventh Part of the Scientific Memoirs the Editor has already acknowledged the assistance afforded to the work by the British Association for the Advancement of Science, concurring, as it has done most efficiently, with the other public bodies and individuals by whom the success of the undertaking had been promoted. It is now his pleasing duty to state, that the support thus given having afforded an opportunity for the plan and objects of the work to become more generally known, the sale has been so far increased as to give an improved prospect of its permanence; and that a portion of the Third Volume is already in the press.

Of the Memoirs contained in Part VIII. the following have been received from the Committee of the British Association for procuring the translation and publication of Foreign Scientific Memoirs, viz.:—

The Galvanic Circuit investigated Mathematically. By Dr. G. S. OHM. *Continuation.*

BESSEL on the Barometrical Measurement of Heights.

RUDBERG on the Expansion of Dry Air.

WEBER on a Transportable Magnetometer. With a Plate.

WEBER on the Magnetic Term-Observations for 1839 of the German Magnetic Association. *Extract.* With a Plate.

GOLDSCHMIDT on the Observations of Magnetic Declination at Göttingen.

The continuation of the translation of Ohm's Memoir has

been paid for out of the grant at the disposal of the Committee; as have the Plates for the two Memoirs of Professor Weber.

The translation of Rudberg's experiments has been presented to the Committee by Professor W. H. Miller, of Cambridge; and the translation of the Memoirs of Bessel, Weber, and Goldschmidt, by Major Sabine; and by the Committee to the Editor.

The Editor has also to acknowledge the valuable assistance which he has received from Professors Miller and Wheatstone in the revision of the translation of Ohm's Memoirs, and of Professor Graham and Richard Phillips, Esq., for similar services with regard to the Chemical Memoirs. To the friendly and zealous cooperation of Major Sabine he is also most especially indebted.

Red Lion Court, Fleet Street,
Feb. 20, 1841.

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TO ILLUSTRATE THE FOLLOWING MEMOIRS.

SCIENTIFIC MEMOIRS.

VOL. II.—PART I.

Article I.

Electro-Magnetic Experiments, forming a Sequel to the Memoir on the Application of Electro-Magnetism to the Movement of Machines; by M. H. Jacobi, *Doctor of Science, and Professor at the University of Dorpat**. *Second Series*†.

[From a Pamphlet kindly communicated by the Author.]

24.

The question relative to the magnetic power of hollow, soft iron tubes, magnetized by an electrical helix, occupies an important place among the numerous questions presented to us in relation to the employment of the mechanical force of electro-dynamic magnets. A short time previous to my departure from Königsberg, I had made preparations to undertake a series of experiments on this subject. I was happy to find Professor Parrot engaged in these researches, the important results of which will be found published in No. 16 of the *Bulletin de l'Académie*. This distinguished philosopher had the kindness not only to communicate to me part of these results previous to their publication, but also, at my request, entrusted to me the tubes described in his memoir, in order that I myself might institute similar experiments. These experiments are the subject of the following paper, which I have the honour of presenting to the illustrious Academy, requesting that they will judge of them with indulgence.

* Translated by Mr. William Francis.

† The First Series, together with Memoirs by Prof. Botto, Dr. Schulthess and Prof. Henry, relative to this subject, are to be found at p. 503 of the First Volume.

25.

If we first consider the experiments enumerated by M. Parrot, p. 123 of the *Bulletin*, it appears that we may deduce from them the important result, that the solid thickness of hollow tubes has but little influence on their magnetic power. In fact, the magnetism of the tube designated by *B*, of $2\frac{1}{2}$ lines solid thickness, differs merely $\frac{1}{90}$ in magnetic power from the tube *D*, which is but one line in thickness. Even the energy of the solid cylinder of equal surface is far below the proportion relative to its mass. These experiments are connected with the beautiful known law established by Mr. Barlow, and which relates to the action of terrestrial magnetism on solid and hollow soft iron spheres, a law which is also confirmed by the experiments of Captain Kater on sheet-iron tubes, and which has undergone the rigorous and scientific calculations of M. Poisson. Mr. Barlow fixed the limit of the solid thickness at about $\frac{1}{30}$ of an inch; such is the cylinder *F* of M. Parrot, constructed of common sheet-iron, of $\frac{2}{45}$ of an inch in thickness, and the result of which differs considerably from other experiments. Its magnetic power amounted only to $32\frac{1}{2}$, while that of the tube *D* of one line in solid thickness is expressed by 89.

26.

We ought to observe extreme caution in drawing conclusions from the experiments in question. The manner of comparing magnetic forces from the adherence of the armature is itself subject to serious inconveniences, which consist, for the most part, in the complicated accidents which accompany the contact of the armature and its sudden interruption. The errors which proceed from this cause being very considerable, it is at least worth while to endeavour to separate them from others depending on the continual variation of the voltaic action, a source not less prolific of serious errors. The somewhat difficult experiments instituted on the magnetic force of a soft iron bar, bent in a horseshoe form, and which I have related in Art. 9. of this memoir, made me sensible of these inconveniences in all their extent. But they may easily be overcome, by keeping an exact account of the force of the current, by means of a galvanometer interposed in the circuit. I had constructed, purposely for such experiments, an instrument of very great service. A needle,

four inches in length, balanced on a very fine pivot, at the centre of a circle divided into semi-degrees, is placed in a box, furnished with three adjusting-screws, so as to secure a horizontal position. Instead of the usual multiplying coil, which would too much weaken the current, there is only a single brass wire, of $1\frac{1}{4}$ line in thickness, which passes exactly through the centre below the needle. The extremities of this wire are furnished with two small cups containing a few drops of mercury, and which allows the circuit to be easily completed. The bottom of the box consists of a glass plate, not a metallic one, as it is usually made; a precaution which I have thought necessary for magneto electric experiments. This galvanometer is nevertheless so sensitive as to be affected by very weak electric forces. Although the deviations of the needle cannot serve as an exact measure of the force of the current, they however indicate the slightest variation the battery has undergone during the progress of the experiments. By employing a single voltaic pair, of small dimensions, as Professor Parrot has done, there are many means of restoring, at any time, the original state, and of having a constant force. Lastly, with respect to the importance of these experiments, it would be no great loss to reject a pair of plates which have become worn, and to substitute fresh ones in their place. Moreover, we may convince ourselves that it is far more necessary to keep account of the temperature of the conducting liquid than to clean the pair after each experiment, &c. But whatever pains we may take to operate under similar circumstances, we must recollect that the experiments cannot but be incorrect, unless we employ the galvanometer.

The most sure and exact method for these experiments, is to try the magnetic power of two or three bars at a time. The helices are united into a single conjunctive wire so as to submit these bars constantly to the influence of the same current. By interposing a galvanometer, which is indispensable, any variation in the action of the battery may even be turned to account; for by this means, we learn what relation exists between the relative power of the different portions of the circuit and the intensity of the current. Trifling differences in the construction of the helices may be removed by some method of elimination or by reciprocal combinations. The details into which I have entered will, I trust, be excused, as it is often impossible to

avoid them when we are examining closely, by different processes, subjects in which we are deeply interested.

27.

However these experiments may be conducted, one very important consequence results—namely, that the above-mentioned law of Mr. Barlow must be limited to very weak inductive forces, as is the case with terrestrial magnetism. In employing more energetic electric currents we find a more considerable difference between the magnetic power of tubes of various thickness. For instance, in considering the cylinders *B* and *D*, the weights of which are as 2 : 1, we shall find that their magnetic power, under the influence of a weak current proceeding from a pair of $2\frac{1}{2}$ square inches, is as 90 : 89 ; on employing a pair of plates 24 inches square, as 90 : 82 ; and with a pair of a 100 inches, as 94 : 76. Thus, with respect to the relation existing between the magnetic power and the masses, there will always be a great advantage in employing hollow tubes for the motive parts of magnetic machines. This advantage will only be destroyed at that point, where the magnetic forces, divided by the masses, become equal quantities; although it is still a great question whether this limit can ever be attained. A separate experiment which I made on the two tubes *B* and *D*, and which I am about to mention, appears to support the opinion, that this limit of constant relation is very distant, or that electric forces of considerable energy would be required to approximate to this term. I employed a voltaic battery, consisting of 12 plates of amalgamated zinc, each of 72 square inches, and inclosed in a copper sheath. The pairs were combined into a battery, and the apparatus was immersed in a trough of baked earth, without partitions, and filled with the mixture of sulphuric and nitric acids, recommended by Mr. Faraday. The force of this apparatus was very powerful. The helix, forming the conjunctive wire, and wound round the cylinder, became highly heated. Under the influence of such a current, the tube *B*, of $2\frac{1}{2}$ lines in thickness, was capable of supporting a weight of 95lb., while the cylinder *D*, of 1 line in thickness, could only lift 51lb. In a second experiment the forces were as 94 : 50. From this it is evident that the advantage is always on the side of the cylinder of least mass. I have purposely avoided bringing the solid cylinder into this comparison. It will subsequently

be seen that there exists a distribution of magnetism in solid cylinders entirely distinct from that in hollow cylinders, so that not only the mass, but the geometrical form also, must be taken into account.

28.

The magneto-electric effects which take place on completing or breaking a voltaic circuit, are, as is well known, much more decided when the connecting wire is bent into a helix, and still more so, if this latter contains a soft iron nucleus. Whether this nucleus was a solid or hollow tube, there was no perceptible difference in the discharge or splendour of the spark of disjunction, even when the wire of the helix $1\frac{1}{2}$ line in thickness, or a second coil of brass wire of 0·5 line diameter was employed. These experiments were performed in the dark, in order to be able to judge well of the splendour of the spark; after each experiment, the surface of the mercury, and the electrode (*réophore*) inserted in it, were carefully cleaned. Moreover, I ascertained the action of the pair, that constructed of zinc and silver, by the interposition of a galvanometer which only indicated a variation of from $35\frac{1}{2}$ to 35, and from $24\frac{1}{4}$ to 24, occurring during the course of the experiments. It is hardly necessary to add, that the spark was scarcely perceptible as soon as the nucleus had been removed.

I made various combinations, which it is unnecessary to describe more in detail, to produce a magneto-electric current by the aid of those tubes, sometimes by employing them as armatures of a horse-shoe, sometimes as the nuclei of a magneto-electric helix. The force of the current was indicated by the deviations of the needle of an astatic galvanometer of Nobili. Although these experiments were sufficiently delicate, considering that the least accidents in the position of the armature exert a certain influence on the deviation of the needle, yet I did not find any difference so notable as to be obliged to attribute it to some influence of the mass of the electro-motive cylinders. The law, therefore, of Mr. Barlow may also be admitted with respect to magneto-electric actions. But repeated experiments are necessary to learn whether there are not conditions analogous to those of Art. 27, and relative to a highly developed magnetic state. I may be allowed to add that, had there been any notable difference in these experiments, it might justly have been concluded that the magneto-electric current, or the extra-current

which gives uniformity to the course of the magnetic machine, might be considerably weakened by employing hollow tubes (18).

29.

The free magnetism of the inner surface of hollow tubes magnetized by an electric helix is very feeble, although the magnetization of the external surface is very energetic. I shall return to this subject subsequently, but shall confine myself at present to the remark, that this magnetism does not appear to belong properly to the inner surface, but that it should be attributed to the external layer which acts at a distance, on the testing needle, or on the iron filings which have been introduced into the interior (36). If we employ a hollow tube for the armature of a horse-shoe, the magnetism of the interior is much more decided; but the distribution at the surface is effected in so complicated a manner that I have not yet been able to account in any way for it. If we break the voltaic circuit which serves to magnetize the horse-shoe, the armature remains adherent to the poles; unless it is a hollow tube, every trace of free magnetism disappears. When the armature is solid, it still retains some traces of magnetism, which appear to be diffused rather at the lower surface than at the upper part. The force with which the armature still adhered, after having interrupted the voltaic circuit, was for the cylinder *D* of one line in thickness $11\frac{5}{8}$ lb., and for the solid cylinder only $7\frac{1}{3}$ lb., their own weight included. These numbers are the arithmetical means of five closely related experiments.

30.

A number of interesting questions are naturally connected with researches of this kind, among which, those respecting the state of magnetic saturation of which soft iron is capable, occupy an important place. It is customary to use the expression "magnetized to saturation," chiefly when it relates to the rendering of steel magnetic. But can such a state also exist in soft iron magnetized by the influence of currents, whose energy and quality may be increased at will? Is there a limit above which magnetic development cannot be forced? In fact we may expect from experiments undertaken on this subject, brilliant and fertile results standing in closer connexion with the essence of these problematic forces. But whoever has devoted himself to expensive and wearisome researches of this kind, will

confess that isolated experiments will not lead to the object in view. For this purpose, well followed up experiments, directed by the knowledge, zeal, and the means at the control of a scientific body, are necessary. I do not believe that Europe can offer elsewhere a union of circumstances so favourable for grand scientific enterprises as is presented by the illustrious Imperial Academy. I submit my desire to advance experiments in magneto-electricity to this distinguished body, whose endeavours are directed to the enlargements of the limits of each branch of science in which it engages, and to extend its boundaries.

We have no right to suppose that the limits of the magnetic power which soft iron is capable of acquiring, are narrowly restricted, or that they have ever been attained by experimentalists. I have made on this subject an experiment which is not without interest. The ends of the helix surrounding the horse-shoe I have described (Art. 9), were placed in contact with a battery of 16 pairs of 72 square inches, recently constructed, and charged with nitro-sulphuric acid (27). The armature, which weighed 3 lb., was placed horizontally, and at a distance of 5 inches below the extremities of the horse-shoe. As soon as the circuit was completed, this armature raised itself and sprung with such violence towards the ends of the magnet, that we might, without exaggeration, estimate the force as equal to that acquired by falling 15 to 20 feet. The result of a second experiment was somewhat less, and after repeated experiments it was necessary to diminish the distance to within two to three inches, to cause the armature to spring towards the magnet with so powerful an energy. This beautiful experiment deserves being repeated, but for its success a fresh and powerful battery is necessary. For the present, we must be content to measure in one way or another the mechanical force of the magnetism, or the useful work $\left(\int_0^h M\,ds = \frac{P\,v^2}{4\,g}\right)$ at our disposal, in the case of the attraction of the armature. The mode of operation of this force renders it very difficult to find an exact measure to express the effects, due for the most part to the active power produced by an accelerating force, the law of which is not exactly known.

With respect to the state of magnetic saturation, we shall never attain it, unless by augmenting at the same time the intensity and number of the currents, to the influence of which the

soft iron is subjected. I shall take the liberty of making some remarks on this subject. In examining the phænomena presented by the conjunctive wire, we see that the beautiful theory of M. Ohm completely accounts for them. Considerably enlarged by the ingenious researches of M. Lenz, and conjointly with the electro-chemical views of Mr. Faraday, this theory has become capable of connecting under one sole point of view a multitude of facts. But, nevertheless, the elements serving as basis to this theory are not placed beyond objection. The resistance opposed by any conductor to the passage of the electric current is there admitted as a permanent force, and enters as such into the general expression of the force of the current. Let E be the electro-motive force, R the resistance of a pair, and R' the resistance of the conjunctive wire, the force of the current, measured in any way whatsoever, will be expressed by $\frac{m\,n\,E}{m\,R + n\,R'}$, and this force will increase indefinitely by multiplying at the same time the surface n and the number m of the pairs. But it would not be necessary to excite to a great degree the energy of the battery, in order to destroy the conducting wire by the development of heat, or rather by the heat which the wire itself developes, in opposing the passage of the voltaic current. Certain powerful effects, which do not take place suddenly like other physical phænomena, (for instance, the solidifying of water when its temperature has sunk below zero), but which accompany all electrical actions even from their most feeble indications, and which are always directed towards the weakening of the conducting power, must not be neglected when the nature of the conducting wire has to be taken into account. M. Lenz, in his valuable memoir on the conducting power of metals at various temperatures, has drawn the attention of philosophers to the complication of effects caused by the influence of the temperature of the conducting wire; the power of the current, the temperature, and the resistance being in an intimate and reciprocal relation. In another memoir, this philosopher has announced some important facts relative to the conducting power, which is changed by the least difference in the chemical or physical conditions of the metals; so that this power, measured with precision, the type of which has been given by this able physicist, may serve as the most delicate test of the purity of the metals. I cannot, moreover, pass unnoticed the re-

markable experiments of M. Peltier on the calorific phænomena of electricity in a conductor composed of different metals*. But if we collect all the isolated facts on this point, everything leads us to believe, that the resistance opposed by a conductor to the passage of voltaic electricity is nothing else than a reactive thermo-electric current, whose power increases with the elevation of the temperature, and especially with the heterogeneity of the conducting mass, which may be regarded as wholly composed of thermo-electric elements. According to this hypothesis, which must however be confirmed by experiment, this resistance would be null in a homogeneous body.—In general, matter opposes the transmission of physical forces, which tend to produce its disintegrations. It gives rise to, or generates of itself, forces which are frequently of the same nature, and tend to restore all molecular derangement occasioned by the primitive force. It is a struggle which terminates by the production of some state of equilibrium, or by the total destruction of the conducting body; but it would never end in producing any state of saturation.

That which is evident in regard to the electric force traversing any body whatsoever, cannot be admitted in regard to magnetism without some reserve; nor can it be supposed that soft iron can, without being affected, become the depository of a force not less extraordinary and not less energetic in producing thermal and chemical actions. In fact, soft iron possesses the power of generating a magnetism opposed to that which a current of induction tends to cause it to adopt, and I do not think that the magneto-electric current of Mr. Faraday can be otherwise conceived than as such a reaction. But, although the duration of this reactive current is not infinitely small, as has been sufficiently proved by the mechanical effect it exercises on the needle, nevertheless we shall not succeed in producing a continued magneto-electric current by means of latent magnetism. In fact, it would then be a state of equilibrium, or a limit of the magnetization. The uniform process of the magnetic machine (18), and of almost all the magneto-electric rotatory apparatus†, is owing, for the most part, to an analogous state of equilibrium between the voltaic and the magneto-electric

* See Becquerel's Treatise on Electricity, vol. iii. Article 444.

† In the Treatises on Physics, the uniformity of the movement of these apparatus, of Barlow's wheel, &c., is attributed to the resistance of the air, and of the mercury, which increase with the velocity. Since the grand discovery of Mr. Faraday these have been shown to be not the only causes.

currents generated by the same movement; but such a state can never occur to magnetism at rest.

There is probably also another state of equilibrium which may be attributed in part to magneto-electric currents. In the numerous experiments which I have made, I remarked, conformably to M. Ohm's formula, that the action of a voltaic current is much more constant when a rather long conducting wire is employed. I communicated some time back to Professor Moser of Königsberg, the idea, that under similar conditions the action of a pair ought to be of higher constancy, if long conducting wire be wound spirally round a soft iron bar, than when this same wire is extended or inverted on itself. The usual decrease in the energy of the battery would give rise to a magneto-electric current of the same direction as the voltaic current, and serving partly to re-establish this latter. I tried this delicate experiment, but have not yet been able to arrive at incontestable results. I shall recommence these researches, which appear to me to be of importance for the validity of the theory, and fertile in its practical applications.

The following experiment, which I owe in part to Professor Moser, appears to me to throw a great light on the intimate nature of magnetism. The helix of one arm of the horse-shoe (Art. 9) was brought into contact with a voltaic pair, while the helix of the other arm could be united to the multiplier of a very sensible galvanometer. This latter circuit being completed, there is, as is well known, a very energetic deviation of the needle, as soon as the voltaic circuit is completed or interrupted. But after the establishment of the voltaic circuit, if 4 to 5, or even 7 seconds, are allowed to elapse before completing the magneto-electric circuit, there will still be a sensible deviation of the needle. The same observation may be made, but in an inverse manner, if the voltaic circuit be interrupted some seconds previous to the establishment of the magneto-electric circuit. The deviation weakened in proportion to the length of time which has elapsed between the two operations. These effects are much more decided, if an armature be connected with the extremities of the magnet*, and are probably still more so when a soft iron ring is employed.

* I have just noticed in the 6th part of Poggendorff's Annalen, that Professor Magnus, of Berlin, has made a series of analogous experiments.

31.

In explanation of the fact that the magneto-electric currents are of a certain duration, great caution must be taken not to adopt that hypothesis which most easily presents itself; for it is very rare that such an explanation is correct, or even most probable. We shall certainly conclude from it, that some time is required for the soft iron to attain the maximum of magnetic force, or for the development of the magnetism to become any function of the time. The mathematical idea of a force requires that it have an existence independent of time. This is the case with celestial attraction; as soon as a planet appears opposite to the sun, the force of attraction, in conformity with its actual position, takes place according to the Newtonian law; no time is necessary for the entire mass of the body to become seized by gravitation. Here is the type of a force which serves as a basis, if we wish to form a general and correct idea of it. But when a force acts on any system of material points, and if we substitute in place of the primitive effects, the active force this system has acquired, this latter force will be a function of the time during which the action of the force was in operation. This is incontestable, but I think it would not be possible to hazard the inverse conclusion; for instance, where we perceive the development or the propagation of a force requires time, it is a molecular movement, or the movement of any matter taking place. Thus, if we suppose that the development of the magnetism be not instantaneous, we must admit, either that the magnetism consists in the movement of a fluid, or of some æther, or that the magnetic force produces some molecular movement, or positive displacement of the particles of the soft iron. I am of the latter opinion; the time required for the propagation of magnetism being too considerable to allow of attributing this force to the movement of an æther, analogous to the luminous or the electric æther, the rapidity of which, according to Mr. Wheatstone's experiments, surpasses even that of light. There is nothing to oppose this, in the supposition of a positive contraction taking place in the soft iron as soon as it is exposed to magnetic influence. The armature which is attracted by a force equivalent to a weight of 1000 lb., would exercise the same mechanical effects as a weight of the same magnitude attached to an armature of a non-magnetized horse-shoe; and it is not needful to employ very delicate instruments to estimate the compression produced by such a charge.

We are acquainted with the striking and perfect analogy existing between magnets and electro-dynamic cylinders. There is a very beautiful experiment performed by Dr. Roget, and which might serve as a confirmation to these arguments; I relate it in his own words: "It occurred to me, soon after hearing of Ampère's discovery of the attraction of electrical currents, that it might be possible to render the attraction between the successive turns of a heliacal coil very sensible, if the wires were sufficiently flexible and elastic; and, with the assistance of Mr. Faraday, this conjecture was put to the test of experiment, in the laboratory of the Royal Institution. A slender harpsichord wire, bent into a helix, being placed in a voltaic current, instantly shortened itself whenever the electrical current was sent through it, but recovered its former dimensions the moment the current was intermitted." I hope soon to be able to communicate to the Academy the result of the experiments I shall institute on this subject, conjointly with its illustrious member M. Struve, who has promised me his assistance, these experiments requiring the exactitude and delicacy of micrometrical observations. When we consider the electro-dynamic cylinder, its total effect increases by the reciprocal attraction of separate coils, whose action becomes less oblique. The same is the case with soft iron, the magnetism of which increases up to a certain limit, by the action of contraction. Heat everywhere presents itself as the enemy of magnetism; perhaps it is because the two forces encounter each other in opposed molecular actions. For the present I abstain from following up these discussions, and all connected with them.

32.

Although the remarkable effects occurring at the instant when voltaic contact is completed or interrupted, bear a striking analogy to the actions of magneto-electric currents, there exists, however, a marked difference. If a conducting wire, covered with silk, be bent back on itself, so that the direction of the current is opposed in the adjacent parts, (Faraday, 9th Series, Art. 1096) there is neither discharge nor spark at the moment of the disjunction, the opposite currents of induction arranging themselves in equilibrium to annul these effects. If we remove the adjacent parts, the discharge and spark take place. This experiment made me think that it was also possible to destroy the effects of a magneto-electric current, by

causing it to pass through a wire bent back on itself; this however, is not the case. For this purpose I employed a helix formed of two distinct wires, each of 400 feet in length (22); the extremities of the one wire being designated $a\ a'$, and those of the other $b\ b'$, I could not observe any notable difference in the deviation of the needle of the interposed galvanometer, whether the magneto-electric current was caused to pass through these two wires in the direction $a\ a'\ b'\ b$, or in that of $a\ a'\ b\ b'$. In this relation, the magneto-electric current of short duration appears to have more analogy with the voltaic current, which possesses the property, as has been proved by Professor Faraday, of not being affected, either by the manner in which the wire is bent, or by the presence of soft iron serving as nucleus.

This will perhaps be the proper place to relate the following experiment which I performed. Every one is well acquainted with the remarkable arrangement, presented by M. Becquerel to the French Academy of Sciences, in its meeting of the 7th December, 1835 (*L'Institut*, No. 135). Assisted by Professor Göbel, I constructed a similar battery, which developed a considerable quantity of gas at the platina plate immersed in an alkaline solution. When the galvanometer had been interposed, there was a deviation of 22°, which lasted for 6 to 7 hours, and which was not much diminished by the interposition of a helix of 800 feet; but on breaking the circuit I observed neither discharge nor spark. Nor was even the least sensation felt on interposing, as a secondary circuit, the tongue, or any flaying of the skin. To institute a comparison, I constructed a very small pile of copper, of zinc, and very diluted sulphuric acid, of which I set only sufficient surface in action to obtain the same deviation of the needle as with Becquerel's arrangement. With this very small battery of ordinary construction, the spark was visible, and a very feeble discharge was felt by the hands when moistened with acidulated water. I repeated several times the first experiment, and do not believe that there was any error in the mode of experimenting; so that we may conclude that, if there exist an electric current proceeding from the combination of bodies, it possesses very different properties from that which takes place during the decomposition of an electrolyte.

33.

During my late stay in Berlin, in September of last year, (1834), Professor Dove, Member of the Royal Academy of

Sciences of Berlin, stated to me that he had not obtained the least development of magnetism by means of a magneto-electric helix, which he had introduced into a piece of a gun-barrel. I was then of the opinion of this philosopher, that this negative result must be attributed to some accidental property of the iron; the more so, as a helix which he had coiled round the barrel had not produced any considerable effect. M. Dove does not appear to have published any additions to this isolated experiment. Altogether independent of this previous experiment, M. Parrot has made the discovery which he has published in his memoir, (*Bulletin scientifique*, No. 16) that a magneto-electric helix, placed in the interior of a hollow cylinder, does not communicate any magnetic force to the soft iron. This coincidence of observations does not allow of the admission of any accidental state of the iron which has been exposed to the influence of the current. We might probably arrive at the explanation of this curious fact, by following up the electro-dynamic views of M. Ampère on the constitution of magnets. Regarding the magnets as an assemblage of elementary wires, parallel to the axis, and surrounded by electric currents, which are all in the same direction relatively to the axis, M. Ampère is obliged, in order to comply with known facts, to adopt a covered surface, which represents the total effect of these isolated currents, or of those solenoids which may be substituted in their stead. In conformity with experiment, it is unnecessary to take any notice of the inner currents, as they for the most part destroy each other. The direction of the magnetization is determined by that of the currents; so that if we suppose the magnet to be directed by the action of the earth, the currents proceed from east to west in the upper part, and from west to east in the lower part. This is merely a small omission which has escaped this illustrious philosopher, if he has not specially added that, in order to be able to replace the elementary effects by the magnetic covering, it is absolutely necessary to admit also the condition, that the enveloped currents have, relatively to the axis of the whole body, the same direction as the elementary currents have relatively to the axis of the wires. It is this condition, however, which constitutes the distinctive character of a magnetic surface. For it suffices to look at fig. 1. in Pl. I. to judge of the discordance of these directions relatively to the inner surface, and of their concordance relatively to the external surface. The experiments of Mr. Barlow, and the calculations of M. Poisson, to

which I have alluded in Art. 25., support this mode of considering the magnetic state of the inner part of concave surfaces. The electric helix, and this concave surface, are nearly in the same relation as two juxta-posed magnets, with poles of opposite name. There is no manifest effect; it is merely a disguised magnetic state.

34.

The experiments of M. Parrot, which he made only with a battery of small dimensions, led me to think at first that the development of the magnetism on the inner surface was only very weak, and that, on employing an arrangement sufficiently energetic, the magnetization would be shown in a more decided manner. This, however, was not the case; for the pile of 16 pairs (30), which sensibly heated the helix, had not the least effect. Notwithstanding this powerful current, there was still not a trace of magnetization in a soft iron wire of a millimeter in thickness, which had been connected to the external surface of a helix parallel to its axis. On detaching it, and introducing only one of its extremities into the interior, and to the centre of the helix, the wire became strongly magnetized. In this experiment, care must be taken not to submit the wire to the influence of the helix, after having completed the circuit; for the effects of induction which accompany the moments when contact is established or broken, affect the steel (37), and the soft iron made into wire, and which has partly taken the nature of steel.

35.

In electro-magnetic experiments, the question as to indefinite currents frequently occurs; an expression which, in certain cases, should not be employed without modification. In fact, this expression is inexact, for it is especially characteristic of a voltaic current to return to its origin, a condition which must never be lost sight of. Each closed circuit possesses an axis which bears the same relation to electric or magnetic elements, as the axis passing through the centre of gravity does to the mass of the body. These two axes will coincide in symmetrical and homogeneous circuits. On exposing soft iron to the inductive power of indefinite voltaic currents, we must take into account the position of the electrical axes, so as to be able to predict whether there will be an effect of magnetization or not. *A*, *B*, (Pl.I. fig. 2.), being any indefinite current, the bar *M* cannot become

magnetic, unless the centre *C* of the voltaic circuit is situated on the same side as the bar, or that this latter is placed at the interior of the circuit. The following experiment may serve to confirm this opinion.

A wooden box, *A B C D* (Pl. I. fig. 3), 3 feet in length, and $1\frac{1}{2}$ foot broad, was surrounded with six convolutions of brass wire. The ends of this wire being placed in contact with a single voltaic pair, a small bar of soft iron, placed in *M*, was sufficiently magnetized to attract a quantity of iron filings. But in placing this same small bar on *M*, there was no trace of magnetism even on employing the powerful battery of 16 pairs. This simple experiment appears to me to be sufficiently important to render the facts intelligible. We might thus lay down the general rule; that if we considered any body magnetized by induction, there will be a development of magnetism only at the surfaces, whose rays of curvature traverse the body, or bisect it according to its thickness. If the rays are obliged to be prolonged in order to bisect the body, there will be no magnetism evident. Moreover, the currents must always be so directed, that the body shall be situated in the interior, and surrounded by the circuit. When a plate of soft iron, or of sheet-iron, is bent in any way whatsoever, and is exposed to the influence of electric currents, the curve of inflection *A* and *B* (Pl. I. fig. 4) are at the same time the limits of the developed magnetism. The iron filings will attach themselves only to the extremities, or convex surfaces, as indicated by the figure. I did not consider it necessary to make this experiment, for there can be no doubt of its being the case. This manner of considering these relations also explains why soft iron employed to complete a voltaic circuit, acts like all other metals, and is not magnetized transversally or according to the axis of the currents, A plate of sheet-iron *F* (Pl. I. fig. 5), of half a line in thickness, and curved as shown in the figure, was soldered to a voltaic pair of $3\frac{1}{2}$ inches in length; on plunging it into acidulated water, a testing needle was slightly affected, but not more than by a current of the same intensity which had been passed through a conjunctive wire of copper.

36.

M. Parrot seems to be of opinion, that an interior helix not only adds nothing to the development of magnetism, but that it even exercises some weakening action, whatever may be the direction of the current. In truth, this would be very remarkable!

a kind of anti-magnetism! I have not succeeded in confirming this discovery by the following experiment. In the first place I considered it necessary to exclude the weakening effects, which constantly take place when a second uniting wire is introduced into a closed circuit; and it is easy to attain this by employing also another battery, which is placed in contact with the extremities of the interior helix. Now the hollow tube having been rendered magnetic by a surrounding helix, a considerable quantity of iron filings adhered to the poles. The contact of the interior helix, with its own battery, was then established. Not a single grain of the filings fell off, whatever was the direction of the current;—a proof that more experiments are requisite on the part of M. Parrot to confirm this discovery. This is also another experiment, which proves incontestably that the inner surface of a hollow cylinder is entirely deprived of magnetism (29). The two extremities of an inner helix were placed in contact with a very delicate galvanometer; the cylinder was then attached as armature to a horse-shoe strongly magnetized. The adherence was very powerful, and yet the needle was not in the least affected. But when one of the ends of a small bar of soft iron was introduced into the inner helix, the needle was greatly agitated as soon as the other extremity was placed in contact with one of the poles. The hollow cylinder had been rendered magnetic by a surrounding coil; there was in this case also no deviation of the galvanometer placed in contact with the interior coil.

37.

To these observations relating to soft iron magnetized by influence, it is necessary to add a few words with respect to steel, this being endowed with a coercitive force, supposed to be null in soft iron. Subjected to inductive currents, sufficiently energetic to overpower this resistance, steel retains the magnetic condition which the currents have caused it to adopt, even when these latter have disappeared. In fact, currents of short duration, which have no influence on soft iron, magnetize considerably tempered steel. If we refer to the soft iron wire subjected to the experiment, described in Art. 34, the wire does, it is true, become magnetic, but the magnetism cannot be developed in presence of the helix, which is endowed with a contrary magnetism. If these two portions could be separated, there would be an evident magnetization; but this cannot be effected, as the mag-

netic fluid, or the currents developed by the influence of the currents, reunite as soon as the soft iron is removed. The coercitive force opposes the union of these fluids or currents, and retains them in the position of equilibrium they have assumed. In soft iron, the effect of induction might be considered general; the mass, or rather the entire surface, is equally attacked; in tempered steel the magnetism can fix to such and such spot as is capable of retaining it. It is similar to the electrical condition of isolating substances. The analogy between the figures of Lichtenberg and those of Haldat cannot be misconceived. After this, it would not be astonishing to see the magnetizing of steel, by means of very weak electric forces, under the very conditions which leave soft iron intact, even under the influence of highly energetic forces. Four steel needles, of $0^{m}\cdot1$ in length, and of a thickness of $6^{m}\cdot001$, were fixed symmetrically by their ends to the outer surface of a coil which formed a voltaic circuit conjointly with a magneto-electric helix wound round a horse-shoe. On breaking or perfecting the circuit in *g* or *h* (fig. 6), a magneto-electric current is constantly obtained, sometimes in the voltaic direction, at times in the inverse one. The magnetization of the horse-shoe being very weak, on account of the current dividing itself between the two helices, the magneto-electric current of contact or of disjunction, is also weak. Notwithstanding this, the needles exposed to the shock of a single disjunction were powerfully magnetized, all in the direction which had been previously determined, in conformity with the established law. The following table exhibits the magnetic forces acquired.

	Duration of an Oscillation	
No.	Before Magnetization.	After Magnetization.
1.	20″·75	3″·62
2.	26″·25	2″·72
3.	18″·25	2″·9
4.	15″·66	3″·3

It is still necessary to add, that the needles had been purposely arranged so that the poles would be reversed, and this always ensued. Moreover, whilst the voltaic current was actually passing, there was no evident magnetism in the steel needles, as was the case in the soft iron wire.

38.

The law established above (35), and relative to the magnetic condition of the interior of concavities, is neither confirmed nor

shaken by the following experiment. I had a steel cylinder constructed, bored longitudinally in the direction of the axis; it was $0^{m}\cdot13$ in length, $6^{m}\cdot002$ inner diameter, and $0^{m}\cdot005$ solid thickness. This cylinder was suspended by several silk threads not twisted. Its inherent magnetism was very weak, the duration of an oscillation amounting to $103''\cdot5$. The inner surface was exposed to two magnetic bars which were only $0^{m}\cdot004$ in thickness, and were made to slide with great caution in the direction of the inner angles, as is usually done in magnetising by the double touch. Four poles, placed symmetrically, having been rubbed each ten times, the effect of these forty frictions was to reduce the duration of an oscillation from $103''\cdot5$ to $31''\cdot1$. The operation having been repeated, but with the other poles, distanced from the first 45°, the duration of an oscillation was reduced to $26''\cdot95$, and after a third repetition, to $25''\cdot7$. Repeated frictions had no more influence on the duration of the oscillation, which might, however, still be reduced to $23''\cdot1$, by rubbing the outer surface in a similar way. This experiment proves that, in fact, the inner surface is susceptible of being rendered magnetic, but we must suppose that the separation of the magnetic fluid takes place only in the rubbed portions, as is the case in the experiments of M. Haldat. A magnetism diffused uniformly over the entire surface cannot be admitted. This experiment, simple though it be, it seems to me, may nevertheless contribute to establish our notions as to the distribution of magnetism.

As to the action of an electro-dynamic helix placed in the interior of a hollow steel cylinder, and traversed by magneto-electric discharges, no variation in the magnetic condition was perceptible. Care had been taken to isolate the helix as well as possible, and to destroy in part the magnetism which the cylinder had acquired. The duration of an oscillation amounted to $36''$, and this time did not vary even when a succession of discharges in the same direction were maae o pass through the helix. I was not able to try more energetic discharges: but the development of the magnetism being subject to circumstances which depend entirely on peculiar and accidental properties of the steel, it cannot be exactly predicted whether electric currents would or would not produce some effect of magnetization.

Article II.

Results of the Observations made by the Magnetic Association in the year 1836. *Göttingen. Edited by* Carl Friedrich Gauss, *and* Wilhelm Weber*.

[Being the First Annual Report of the Magnetic Association.]

INTRODUCTION.

Among the numerous phænomena of terrestrial magnetism, with which we can only become acquainted by continued observations, accurately performed at various points of the earth's surface, none are in need of a more rigorously systematized cooperation of observers, than the *irregular variations* to which we find this force to be subject. It is sufficiently well known that the Variation, the Dip, and without doubt the Intensity also, (although with respect to the latter, which has but recently been admitted into the circle of inquiries, sufficient observations are still wanting) continually undergo changes—secular changes, which attract our attention only after long intervals of time, but which eventually become very considerable,—and periodical changes, varying according to the yearly and daily period. But for these regular changes, a rigorously systematized cooperation of observers, at various stations, is not essentially necessary, although highly desirable for the purpose of hastening the extension of our knowledge; in these points, every observer, even independently of others, may contribute useful additions.

Such, however, is not the case with respect to the irregular variations to which only of late years a larger share of attention has been devoted. Hiorter and Celsius observed, nearly a century ago, that during the appearance of an *aurora borealis*, the magnetic needle undergoes irregular and, frequently, very great oscillations; and this was subsequently confirmed by numerous observations made by others. Hence it might have been concluded, that the same forces which produce the phænomenon of an aurora borealis act also at the same time upon the magnetic needle; and further, that this action must extend to very considerable distances, since the northern lights are generally visible over a wide circuit. We obtain a still greater notion of the wide extension

* Translated by Mr. W. Francis. The translation has been revised by Professor Lloyd and Major Sabine.

of the activity of these forces from the remark of M. Arago, that frequently on the same days, when he had observed at Paris violent disturbances of the regular movement of the magnetic needle, northern lights, not visible above the horizon of Paris, had been seen at distant places.

The irregularities in the phænomena of terrestrial magnetism, the frequent occurrence of which had also been observed, especially by Humboldt in his numerous observations of the diurnal and horary oscillations of the magnetic needle, thus obtained a peculiar interest. Though the facts which had been remarked, neither proved that all irregular oscillations of the needle are contemporaneous with the northern lights, nor precluded the possibility, that many, perhaps most of them, have merely local causes, yet it was scarcely possible to mistake the evidence of the not unfrequent action, over a wide extent, of great natural forces, which, if they could not yet be investigated in their sources, offered at least a worthy object of natural inquiry, in respect to the relations of their activity and extent.

Superficial and merely accidental recognitions of such relations can bring us no nearer to this goal: in order to attain it, many such phænomena must be contemporaneously followed up in accurate detail at numerous stations, and their time and magnitude closely ascertained and measured. For this purpose, however, previously concerted plans are essentially necessary among those observers who have suitable means at their disposal.

The celebrated philosopher to whom we are indebted for so many additions to our knowledge of terrestrial magnetism was also here the first to lead the way. M. von Humboldt caused to be erected in Berlin, towards the end of the year 1828, a small house, free from iron,—placed in it a variation compass constructed by Gambey,—and concerted with possessors of similar instruments at various places, some of which were very distant, regular observations of the magnetic variation on fixed days. Eight terms in the year were agreed upon, each of forty-four hours, during which the variation was to be noted from hour to hour; at some places observations were made within still narrower limits of time, viz. at every half-hour, or every twenty minutes. The details will be found in the nineteenth volume of Poggendorff's *Annalen der Physik*, *p*. 361; and in the same journal are also the observations which, according to this agree-

ment, were made at the appointed terms, in the years 1829 and 1830, in Berlin, Freiberg, St. Petersburg, Kasan, and Nicolajef, together with the graphical representations of three of them.

In the Göttingen magnetic observatory, which was built in the year 1833, and in which the magnetic apparatus is entirely different in construction from any previously employed, these term-observations were made for the first time on the 20th and 21st of March, 1834; corresponding observations were made in Berlin; but at Göttingen the observations were made every ten minutes, and in Berlin only every hour. Those at Berlin exhibited several considerable movements, which were found also in the Göttingen observations; while these latter exhibited in the intervening times a great number of movements which, of course, were entirely wanting in those made at Berlin. The question, whether the greater part of the fluctuations observed in Göttingen had been merely local, remained therefore still undecided.

The following term of the 4th and 5th of May, 1834, brought with it the decision. The intervening periods were more limited, the observations being made every five minutes, which gave to the results a considerably more definite character. No corresponding observations with Gambey's apparatus during this term, or in any subsequent ones, have been published. On the other hand, M. Sartorius, who had taken an active part in the March term-observations at Göttingen, and who, being on the point of undertaking a journey of several years to Italy, had provided himself with an apparatus similar to the one at Göttingen, but of smaller dimensions, made with it careful and complete observations, at short intervals, during the May term, at Waltershausen, in Bavaria, about twenty German miles from Göttingen. A concordance surprisingly great was manifested, not only in the larger, but even in almost all the smaller oscillations, so that in fact nothing remained which could be justly ascribed to local causes.

During the three following terms, *i. e.* in June, August, and September, 1834, the observations were continued at Göttingen in exactly the same way; and the number of observers at other places, with apparatus either the same or of similar construction, was continually on the increase. Professor Encke, having become acquainted, from personal inspection, with the arrangements in Göttingen, ordered provisionally a similar apparatus of

smaller dimensions for Berlin. M. Sartorius observed with his instrument during all the terms when circumstances allowed, viz. in June at Frankfort, and in September at Bramberg, in the province of Salzburg. Observations were also made in Leipzig, Copenhagen, and Brunswick, with instruments exactly resembling those of Göttingen. The result of the corresponding observations was quite similar to that above mentioned of the May term. Almost all the numerous movements observed at Göttingen occurred in the observations at other places, and although in varied relative magnitudes, yet with a concordance which did not admit of mistake.

In order to obtain further undeniable proof respecting this remarkable result, Professor Weber, being then at Leipzig, arranged that corresponding observations should be made at that place and at Göttingen, and certain hours of the forenoon, noon, and evening of the 1st and 2nd of October were fixed upon for the purpose. These observations, made by highly experienced observers, and with the greatest care, were published entire in Poggendorff's *Annalen der Physik, vol.* xxxiii. *p.* 426, and elucidated by graphic representations. The necessity now became evident of observing the phænomena at much shorter intervals than Humboldt had chosen. We observed during some of the appointed terms at intervals of three minutes, and some other observers did the same. As, however, several of the cooperators adhered to the five minute intervals, and as these in ordinary cases fully suffice, we subsequently, for the sake of uniformity, adopted this as a general rule. But as such small intervals render the labour incomparably more troublesome than the noting from hour to hour, especially in cases in which only a small number of persons can take part, it was necessary, in order to ensure the stability of the Association, to diminish both the number, and the duration of the terms. The number has since been fixed at six in a year, and the duration of each term at twenty-four hours. To each principal term two subordinate terms were added. Other details will be found in the sequel.

The observations have continued uninterruptedly, according to this plan, at Göttingen, and also at a constantly increasing number of other stations. Apparatus of the same or of similar construction to those in Göttingen, are employed in Altona, Augsburg, Berlin, Bonn, Brunswick, Breda, Breslau,

Cassel, Copenhagen, Dublin, Freiberg, Göttingen, Greenwich, Halle, Kassan, Cracow, Leipzig, Milan, Marburg, Munich, Naples, St. Petersburg, and Upsala. From eight of these places no observations have yet come to our knowledge; and, in some others, the participation in the observations, from extrinsic circumstances, has not hitherto been uninterrupted and regular.

Some terms of the earlier period of the Association have been published in graphic representations in Schumacher's *Astronomische Nachrichten*, and in Poggendorff's *Annalen der Physik*. The participation having so much increased, the time appeared to have arrived for taking into consideration a regular publication, in order that the abundant collection of fruitful facts might be made the common property of that portion of the public which is interested in these researches. What we now offer may be considered as the first annual report since the Association has attained a certain extent. From the year 1837, the results of each term will be made public as soon as they can be brought together in a sufficiently perfect manner.

The observations, and their graphical representation, will not merely be accompanied by those explanations and remarks which relate immediately to themselves; but we shall likewise add other memoirs, in which various subjects belonging to the wide field of terrestrial magnetism—the instruments, their use and manipulation, and various applications—will find a place.

With regard to the immediate object of the labours of our Association, the variations of the magnetic Declination, I may be allowed to add one more remark. If, as cannot be doubted, the two other elements of the terrestrial magnetic force, the Inclination and the Intensity, are subject to similar changes, the question may be asked, why such careful labour has been devoted to the first element, in preference, and hitherto exclusively?

The knowledge of the variations and the disturbances of the magnetic Declination possesses in fact a very great practical interest. To the mariner, and the surveyor, it must be of considerable importance to know the frequency and magnitude of the disturbances to which the compass is liable, even were it only to learn what degree of confidence he might place in its indications. For geodesical purposes the future progress of these inquiries may probably do much more. If it is once established that the irregular disturbances are never, or very seldom, merely local,—but that they constantly, or almost always,

occur contemporaneously, and with almost equal magnitude, over great districts, the means are furnished to divest them almost entirely of any injurious practical effect. The surveyor need only make all his operations with the compass accurately according to time, and cause contemporaneous observations to be made at some other not very distant place; and it will be easy to eliminate the effects of these disturbances by comparison, just as travelling observers render their barometrical determinations of height independent of the irregular variations of the barometer, by comparative observations at fixed stations. Of course this has no reference to disturbances of the compass by mineralogical causes.

The preference given to the Declination over the other elements of terrestrial magnetism is less however to be ascribed to these motives than to the present state of our means. The investigation of the laws of nature has for the philosopher its own value and its own reward; and a peculiar charm surrounds the recognition of measure and harmony in that which at first sight appears wholly irregular. In following the constantly varying changes of the Declination, the apparatus at present employed leaves, as to certainty and precision, nothing more to wish; but the same cannot be said of the present means of observation of the other two elements. The time is therefore not yet come for including the latter in the circle of combined inquiry; as soon, however, as the means of observation shall be so far perfected, that we can recognise with certainty, follow with ease, and measure with accuracy, the variations, and chiefly the rapidly varying changes, in the other two elements of terrestrial magnetism, these variations will have the same claims on the united activity of natural inquirers as the variations of the declination now possess. We venture to hope that this day is not far distant. GAUSS.

I.

Remarks on the Arrangement of Magnetical Observatories, and Description of the Instruments to be placed in them.

THE instruments with which the observations were made, which are to be mentioned in these pages, differ in many respects from all previously employed, and a more accurate knowledge of their construction is indispensable, in order to judge of the results

obtained with them. It is true, that what the public have already been made acquainted with on this subject, in former memoirs and notices* might be sufficient; yet the perfect and accurate delineation of these instruments, which we shall give in this place, will render them easily understood, and will, besides, have the advantage that any clever artist can work from it with certainty. Instruments on the plan here represented have been made for Bonn, Dublin, Freiberg, Greenwich, Kasan, Milan, Munich, Naples, and Upsala, by Meyerstein of Göttingen; and those for Göttingen, by Apel, and those for Cracow, Leipzig, and Marburg, by Breithaupt of Cassel, are almost perfectly similar. The description of the smaller instruments which have been employed at some places will be here omitted, since their use has been proved to be less proper, and only to be justified when local circumstances hinder the erection of larger apparatus. Nor will any mention be made of larger instruments, because if they are to fulfil all purposes, they require a proportionally larger place of reception than has hitherto been anywhere assigned to this object.

A long quadrangular *room* which extends about eleven metres in the direction of the magnetic meridian, is best suited for the reception of magnetic instruments. It is not necessary that the side walls should be parallel with this meridian; they may form an angle with it, as is the case, for instance, at Göttingen, where they are in the direction of the astronomical meridian, which at present forms with the magnetic an angle of 18½ degrees. The room must be well lighted, principally from the east and west, and more particularly at the end where the theodolite or the telescope, together with the scale, are to be placed for observation. The room should be protected from currents of air, for which purpose, a double door, and sometimes even double windows, are necessary; and there must be a solid foundation, upon which a *theodolite* and *clock* may be erected. It is also necessary that, from the place of the theodolite telescope, a distant object, the azimuth of which is known or may be accurately determined, may be seen through one of the windows. The floor in the neighbourhood of the instruments, *i. e.* near the centre of the room, must contain no iron, nor must any object containing that metal

* In the memoir, *Intensitas vis magneticæ terrestris ad mensuram absolutam revocata*; *auctore, C. F. Gauss, Gottingæ*, 1833; further, in the *Göttingischen gelehrten Anzeigen*, 1832, p. 2011, 1835, p. 345, and in Schumacher's *Jahrbuche*, 1836, p. 1.

be brought near them. It is even desirable that the entire building, even as to its side walls and roof, should contain no iron; but it is unnecessary to be so cautious as to fear placing a clock, or a theodolite with steel pivots, at a distance of from five to six metres from the instrument. The influence of the steel parts, if they are magnetic, may be approximately deduced by calculation, and is found to be much too small to be sensible at those distances. Small pieces of iron outside the room have still less influence. If, however, there were in the neighbourhood large masses of iron, especially very long iron bars, (such as iron railings), although their influence would be very small, yet it should not be totally neglected. If they are at a distance of a hundred feet from the observatory, they offer no important impediment, at least if they are fixed. Such a locality is sufficient for measuring the Declination and Intensity, and also for observing their changes. Measurements of the Inclination may be performed in the same locality, but not, however, without interrupting the other observations. It therefore appears convenient, when circumstances permit, to assign a separate locality for measurements of the Inclination, which may be at no great distance from the first-named room. Where no absolute measurements are made, but only the changes of Declination observed at the fixed terms, such a room suffices, even should it contain much iron within and without its walls, provided that all the iron remains unmoved during the observations. The room of the Göttingen magnetic observatory is figured in Plate II., and the ground-plan in Plate III.

For the purpose of setting up the instruments, a line should be drawn on the floor representing the magnetic meridian, which line must pass nearly through the middle of the room, and terminate at the southern or northern end of it, where a firm foundation must be made for the theodolite and clock. When this foundation has been prepared, and the theodolite placed upon it, let a *scale* be first attached to the stand of the telescope, so that a plumb-line let fall from the object glass of the telescope passes freely before the scale. The scale must be horizontal and at right angles with the magnetic meridian; it must be capable of being raised or lowered at pleasure, and must be bisected by the magnetic meridian passing through the optical axis of the telescope. Next let fall a plumb-line from the ceiling to the floor, in such manner that the plane of the magnetic meridian passing through this plumb-line may contain the op-

tical axis of the telescope; and when the *magnetometer* is suspended by the plumb-line, the distances of the reflecting plane of the magnetometer (see *mirror* and *mirror-holder*) from the scale and from the telescope, may be together equal to the distance of the telescope from a point on the opposite wall, which is to serve as a mark, to which the telescope may be directed. At the point of the ceiling whence the plumb-line is let fall, the *suspender* of the magnetometer, together with the *elevating screw* and *suspension thread*, must be fixed. Let a weight be provisionally attached to the thread suspended from the elevating screw, in the manner of a plumb-line; adjust the suspender on the ceiling until the thread coincides with the plumb-line, making the length of the suspender parallel to the north or south wall of the room. After this, measure the height of the suspender, of the telescope, and of the scale from the ground. From the first height, subtract half the sum of the two latter, and form a thread of parallel fibres of raw silk (*coconfäden*), whose length is equal to this difference, and which is sufficiently strong to carry the magnetometer, and one kilogramme of additional weight. The upper extremity of this thread is to be fastened to the screw, and the lower to the *stirrup*, (*shiffchen*), in which the magnet bar is placed. A wide *box* is placed under the magnet bar, at the bottom of which are two cushions, upon which the magnet bar would fall, in case the raw silk fibres should break, without endangering the mirror attached to the front extremity of the magnet bar. After these preparations the more accurate measurements may be commenced. These are:

1. To place the magnetic axis of the magnet in a horizontal direction, and the mirror perpendicular to it; or to measure the small angle which the axis of the mirror forms with the magnetic axis.

2. When the magnet is in its mean direction, to bring the force of torsion of the thread to zero, or to measure the small remaining torsion. (Vide seq. *torsion bar.*)

3. To determine the ratio of the moment of torsion of the thread, and the magnetic moment of the bar, in a deflection. (Vide seq. *stirrup* and *torsion circle.*)

4. To ascertain by measurement the place for the mark on the wall opposite to the telescope.

The apparatus is then ready for measurements of the Declination. These consist:

1. In the measurement of the azimuth of the mark.

2. In determining the values of the parts of the scale.

3. In observing the vibrations and elongations. (Vide seq. *quieting bar.*)

More accurate directions for the execution of all the measurements here mentioned will be given in the sequel.

For the measurements of Intensity *measuring scales* are required, by which the position of the *deflecting bar* is determined. These measuring scales may be laid horizontally and parallel to the magnetic meridian, on both sides of the box in which the magnetometer is included, in such manner that lines connecting the corresponding points of the two measuring scales shall be horizontal, and at right angles with the magnetic meridian. The scales should be placed at such height that the deflecting bar placed on them stand at an equal height with the vibrating bar. When this is not the case, the vertical distance between the deflecting bar situated on the measuring scales and the vibrating bar must be measured. The measuring scales must be about 5 to 6 metres in length, and should project an equal distance north and south beyond the magnetometer. If the width of the room allow, it is advantageous to add a third measuring scale horizontally and at right angles with the two former. It may pass under the box of the magnetometer, in such manner that it would be met by a plumb-line let fall from the middle between the centres of suspension and gravity of the vibrating bar. The measuring scales must be so arranged as to allow of displacing them longitudinally, in order to dispose them in such manner, that the deflecting bar, situated at corresponding points, in front and in rear, may produce the same amount of deviation. After these preparations, the measurement of the Intensity consists,

1. In determining the moment of inertia of the deflecting bar. (Vide seq. *weights* and *weight-holder.*)

2. In measuring the time of vibration of the deflecting bar.

3. In measuring the deflection of a suspended auxiliary bar produced by the deflecting bar, at two different distances of the latter, in a south and north, or east and west direction from the magnetometer.

To this general view of the arrangements of the magnetical observatory, and of the apparatus to be placed therein, may be added the following remarks on the separate parts of both.

Remarks on the separate parts of the Magnetic Observatory, and of the Magnetic Instruments.

1. *The room.*—Plates II. and III. are a perspective view and a ground-plan of the room. In the first the southern wall is supposed to be removed; in front, on the right, is seen, *a*, the foundation for the theodolite; *b*, the stand of the theodolite; *c*, the theodolite; *d*, the scale attached to the stand; *e*, the plumb-line suspended from the centre of the object glass. Near to it is stationed the clock, *f*; a line drawn from the theodolite telescope to the mark designated by the arrow on the opposite wall, would represent the magnetic meridian. Towards the centre the suspender of the magnetometer is fixed to the ceiling; from this is suspended the thread carrying the stirrup, in which is placed the magnet bar, to the anterior extremity of which the mirror is fastened vertically. The distance of the mirror from the telescope and its distance from the centre of the scale, (before which passes a plumb-line let fall from the theodolite telescope,) are, together, equal to the distance of the telescope from the mark.

2. *The theodolite.*—For observing the changes of declination, a telescope, having motion in a vertical plane, so that it may from time to time be directed either towards the mirror or towards the mark, is quite sufficient. This movement serves to ascertain and verify the stability of the telescope. For absolute measurements of declination a theodolite is employed instead of such a telescope. As the divisions of a scale divided into millimetres must not only be seen but even their subdivisions estimated, it is necessary that, at a distance of five metres of the scale and of the telescope from the mirror, the telescope should possess a magnifying power of at least thirty.

3. *The clock.*—All observations must be made accurately to time, for which purpose a clock which beats seconds must stand near the observer, with its face towards him. A chronometer may serve the purpose.

4. *The magnetometer.*—Besides a clock and a theodolite, which must be supposed present in all establishments where magnetic observations are to be executed in the most perfect manner, the magnetometer consists of the following parts, which are necessary for measurements of declination:—the magnet bar, the

stirrup with its torsion-circle, the suspender with screw and suspension thread, the mirror and mirror-holder, the torsion-bar, the scale, and the quieting bar; to which must be added, for measurements of intensity, the measuring scales, the deflecting bar, the weights, and the weight-holder. The magnet bar, in its connexion with the stirrup and the torsion-circle, (which again is connected by the suspension thread with the suspender,) and with the mirror and mirror-holder, is represented in Plate X. fig. 3 and 5.

5. *The scale.*—Fig. 10 gives a specimen of the scales hitherto employed, which must be at least one metre in length. M. Rittmüller of Göttingen has lithographed such a scale, and has had it printed on white card-paper.

6. *The plumb-line at the object glass of the telescope.*—A fine wire of dark colour with a weight at its lower extremity, is fastened in such manner to the upper rim of the object glass, that it hangs correctly over its centre. In order to fix this wire, the small notches of the grooved frame of the object glass may be used; or a ring, constructed specially for this purpose, may be slid over the frame, having two slits diametrically opposite each other. The upper slit serves for the fastening of the wire, and the ring is so arranged that the wire passes freely through the lower. If we now view the image of the scale in the mirror through the telescope, we see at the same time the image of this wire projected on the white surface of the scale, and can thus find that point of the scale which lies in the vertical plane of the optical axis of the telescope. The spot where the prolonged plumb-line touches the ground is carefully marked, and serves as a means of testing the immobility of the theodolite stand.

7. *The mirror and mirror-holder.*—The mirror of the magnetometer must be perfectly plane, because otherwise, with a magnifying power of 30, the image of the scale would be indistinct. The plane mirrors from Utzschneider's optical manufactory in Munich have hitherto proved the best. The mirror should be somewhat broader than it is high, as, by the vibration of the magnet bar, the right and left side of the mirror alternately enters the field of the telescope. The best dimensions of the mirror are from 50 to 70 millimetres in height, and from 70 to 100 in breadth. In measuring the distance of the mirror from the scale and from the mark, the refraction of the rays

of light at the anterior surface of the glass must be considered: that plane is the reflecting plane which is equidistant from the anterior and posterior surfaces of the mirror. The mirror is fixed to that end of the magnet bar which is turned towards the telescope, and must form with it so solid a system that no reciprocal disarrangement of either may be feared during the experiment, although the magnet bar be taken out of the stirrup and replaced in it in a reversed position. Moreover, the mirror must have such a position relatively to the bar, that the normal of the mirror shall be quite, or very nearly, parallel to the magnetic axis of the bar. The mirror-holder represented at fig. 4. may serve both these purposes; its frame is attached by screws to the bar. The frame-work supporting the mirror may be turned by screw motion round two rectangular axes, by which it may be brought into the required position.

8. *The suspender, elevating screw, and suspension thread.*—It is very advantageous to fix to the ceiling the thread, which is to carry the magnet bar, as by this it is sufficiently insulated from the floor and protected from all shaking, and because a proper length may in this manner be given to the thread. If, for the support of the magnetometer, we employ, not a metal wire, (the elasticity of which for an equal tenacity is almost ten times greater than that of one formed of silk fibres,) but a thread composed of parallel fibres of raw silk, it lengthens greatly, especially at first; and it is therefore requisite from time to time to raise it, so that the magnet bar and the mirror fixed to it may regain their original height. In raising the thread it is necessary that it should not be displaced in a lateral direction. A screw may be employed for this purpose, in the grooves of which the thread lies, and upon which it can be wound up still further, while the end of the screw works into a fixed nut. The groove in which the thread places itself, by the turning forwards of the screw, takes then, of itself, (from the advance of the whole screw) the place which the vertically suspended line had before occupied. The fixed nut, with solid rest, through which the screw pin passes freely, is let into a wooden slider which is mortised into a large plank fixed to the ceiling, and can slide therein in a direction parallel with the north or south wall of the room. If the position of the magnetic meridian should change in the lapse of time in any considerable degree, this slider will serve to retain the magneto-

meter in the meridian of the telescope. After such a sliding of the suspender on the ceiling, which need be performed but very seldom, it is necessary to place on the opposite wall a new mark, to which the telescope may be directed without departure from the meridian. The thread to which the magnet bar is suspended consists of 200 parallel fibres of raw silk, each of which would support thirty grammes without breaking. The weight which this thread has usually to sustain amounts to nearly 2000 grammes, to which, in the measurements of Intensity, two weights of 500 grammes are added when determining the moment of inertia of the magnetic bar. The thread, therefore, never carries more than half the weight with which it would break. It is about two metres long, and has a torsion force, the moment of which amounts, for small deviations, to about the 1000th part of the magnetic force. The thread may be prepared by winding a single fibre twenty-five times round two glass tubes, distant from each other about four times the intended length of the thread; the two ends of the fibre are then tied firmly together, and the twenty-five-fold skein, thus formed, is stretched by drawing the two glass tubes further from each other. A small hook, carrying a weight, is then attached to the skein, midway between the two tubes, which are then raised and brought together, and the two loops are united in one. Thus a hundred-fold thread is prepared, which forms a loop at top and bottom, and which, being again brought together in a similar manner, forms the thread to which the magnet bar is suspended.

9. *The stirrup and torsion-circle.*—The force of torsion of the thread to which the magnet bar is suspended must not be entirely neglected in absolute measurements of declination and intensity, even though this thread be very long and fine. In order to measure the magnitude of this force, and to diminish its influence, so that the thread in the mean position of the magnet bar may be brought to its natural position when its moment of torsion is zero, it is necessary to be able to turn the thread, at one of its two extremities, round itself, in such manner that the angle of torsion may thereby be measured. In order to have the means of effecting this at hand, the apparatus for this purpose must be at the lower extremity of the thread; but, that the magnet bar may not be turned with it, the stirrup is composed of two parts, an alidade and a circle, which revolve only round a common vertical axis. The alidade supports the magnet bar, and is itself

supported by the circle; the circle is provided with a pivot which passes through the alidade, and has, at its upper extremity, two hooks to receive the pin fixed to the thread. With this arrangement of the stirrup, it is important that the alidade in which the magnet bar lies should rest on the rim of the circle; otherwise, the friction, taking place near the axis of rotation, would produce a displacement of the parts relatively to each other, in consequence of the impulse arising from the vibrating bar. Moreover, the stirrup is so constructed that the magnet bar fits in either on its broad or narrow side. This is done for the purpose of determining accurately, by observations of declination in any of the various positions of the magnet bar in the stirrup, the position of the mirror relatively to the magnetic axis of the bar.

10. *The box and the measuring scales.*—The box which protects the magnetometer from the influence of currents of air is constructed so as to afford ready access to the instrument within. It forms a cylinder of 800 millimetres in diameter, and 300 in height. The cylindrical form is given to it for this reason; in the measurement of intensity, in order to ascertain the moment of inertia, a wooden rod 700 millimetres in length is placed at right angles on the magnet bar of 600 millimetres in length, and this rod, to which weights are suspended, must find a place in the box along with the magnet bar, and must vibrate freely. In order to perform these experiments with convenience, it is also requisite that the box should admit of being entirely opened at the top, and of being tightly closed again, so that there should only remain an aperture at the top for the suspension thread, and one for the mirror at the side. The latter may be closed with a small wooden slider, to exclude air when not observing. The box is closed above by two semicircular lids, which must fit exactly, one of which is provided with a small aperture for the thread. This aperture is not situated in the centre of the circle formed by the two semicircular lids, but is so placed that the thread passing freely through it, the mirror of the magnet bar may hang close before the aperture in the side of the box. This arrangement is necessary, in order that a small aperture may suffice to allow the light to pass from the scale to the mirror, and from that to the telescope. Around the case are fixed the measuring scales on which may be placed a second magnet bar to the south or north, to the east or west of the magnetometer, at prescribed distances

and in a prescribed position, deflecting the suspended bar from the magnetic meridian.

11. *The torsion-bar and deflecting bar.*—That the thread to which the magnet bar is suspended is without torsion in the mean position of the latter, is recognised thus: a brass bar of equal length and breadth, and of nearly equal weight, as the suspended magnet bar, having a small magnet inserted in it (in order somewhat to shorten the duration of the vibration due to the elasticity of the thread) is placed in the stirrup instead of the magnet bar. If the thread is without torsion, the magnetic axis of the small magnet will be in the same line as that of the larger bar was. In order to test this accurately, the auxiliary bar must, like the principal bar, be provided with a mirror and a mirror-holder. For measurements of intensity a second magnet bar of like dimensions to the principal bar is required, which may also be placed in the stirrup instead of the latter, in order to observe its vibrations, and to measure its moment of inertia. The same bar, however, must also serve as a deflecting bar, and for this purpose it is fitted into a small wooden case, which is bounded exteriorly by even surfaces and straight edges parallel to its magnetic axis, in order to give it its place quickly and accurately on the measuring scales.

12. *The weights and weight-holders.*—For measurements of intensity it is requisite that the deflecting bar may also be vibrated, and its moment of inertia thus deduced. For this purpose a thin wooden rod is placed across the vibrating magnet bar, and two equal weights are suspended, at various distances from each other, successively on both sides of the magnet bar. In order to mark the points of suspension, and to determine accurately their mutual distances, both weights, each of which amounts to 500 grammes, are provided with a small capsule. The capsule is placed on a fine point, projecting from the wooden rod. There must be several such projecting points at 50 millimetres distance from one another, with the exception of the two central ones, which are situated at 100 millimetres from each other. These distances must be measured with microscopical accuracy.

13. *The quieting bar.*—In order to perform the observations promptly and accurately, it is of importance to be able to moderate at pleasure the vibrations of the magnet bar; for instance,

when measuring the duration of vibrations, to make the commencing arc no greater than 2 or 3 degrees, and in observing changes of direction, to make the arc as small as possible, never allowing it to exceed 2 or 3 minutes. This end is attained with the quieting bar, in the use of which every observer must practise himself. It is a magnetic bar half the length and breadth, and four times lighter than the principal bar. When this bar is held by the observer behind the theodolite in a horizontal position, and at a right angle with the magnetic meridian, it will cause at this distance (about $5\frac{1}{2}$ meters), if it is strongly magnetized, a deviation of about one minute, westerly if its north pole is held easterly, and *vice versâ*. This deviation becomes smaller in proportion as the bar is removed from the horizontal position, and disappears entirely with its approach to the vertical position. No inconvenience is therefore occasioned by such a bar standing by the wall or near the clock-case (as in Plate II. and III.), till wanted. The use of the quieting bar in magnetic measurements is manifold; and it is important, in order to attain perfect and skilful facility in the performance of these experiments, to become accurately acquainted with its mode of operation. A separate article will therefore be allotted subsequently to the explanation of the rules and laws for its various uses and modes of action.

Finally, the building may be situated in the neighbourhood of other buildings without any injury to the observations. The magnetic observatory in Göttingen, for instance, could not, without causing many difficulties, be situated far from the astronomical observatory. The magnetometer is stationed about 60 metres westward of the astronomical observatory. At this distance moderate magnetic forces exercise so small an influence on the magnetometer, that it has been found unobjectionable to erect in a room of the astronomical observatory an auxiliary magnetic apparatus, which is of very essential service in absolute measurements.

More accurate directions will be given subsequently for determining the influence of a distant magnet, according to its force and position relatively to the magnetometer; and will especially serve this purpose, that when several magnetical apparatus (for instance, a principal magnetometer, an auxiliary magnetometer, and an inclinatorium) are to be fixed in neighbouring

buildings, a positive conviction may be acquired, that their influence on each other is harmless, or, if this should not be the case, that their effect may be reduced to calculation.

Explanation of Plate X.

In this plate the several parts of the magnetometer are represented, with the exception of the clock, theodolite, measuring scales, the box, the torsion and quieting bar, which partly require no particular representation, and in part have been already shown on a smaller scale, in Plates II. and III. On the other hand, the arrangement of the suspender with the elevating screw, the stirrup with the torsion-circle, the mirror-holder, with its corrections, the weights and the weight-holder, stand in need of a more accurate representation, which is given from various sides in this plate, on a scale of half their actual magnitude. The stirrup, the torsion-circle, and the magnet bar in its place, have been represented in three different positions—from the west, from the south, and from above; the mirror-holder, and the suspender, with the elevating screw, have been figured from two sides—from the west, and from the south. In the south view of the stirrup, with the torsion-circle and the magnet bar in its place, is shown the manner in which the weight-carrier may be placed on the magnet bar in a west and east direction, and the two weights, each of half a kilogramme, suspended to the points with which it is furnished, for the purpose of determining, in absolute measurements of intensity, the moment of inertia of the vibrating portion of the magnetometer. To spare room on the plate, the two views of the bearer, with the elevating screw, have been placed in the upper series, close to one another, but this has prevented the bringing of the two into the correct position relatively to the vibrating portion of the magnetometer suspended from them. It is, however, easily seen how the view of the suspender, with the elevating screw in fig. 1., is connected with that of the stirrup, torsion-circle, magnet bar, and mirror-holder in fig. 3. if we attend to the commencement indicated in fig. 1. and the termination indicated in fig. 3. of the vertical line connecting them. These two figures represent the main parts of the magnetometer in a westerly view. In the same manner fig. 2. and fig. 6. are connected, and represent the instrument as observed from the southern position. In fig. 6. the mirror-holder has been taken off from the southern extremity of the magnet bar, so that

it might not conceal the stirrup situated behind it, and is represented by itself in fig. 4. In the westerly view, fig. 3., the small notch in the stirrup into which the weight-carrier fits, is merely indicated; while in the southern view, fig. 6., it is shown as fitted into the notch, and placed on the magnet bar, and the two half kilogrammes it is to bear are suspended from its points.

Fig. 1. presents a view of the suspender, with the screw and suspension thread, from the west. *A A* is a board fixed to the ceiling; *B B* two parallel wooden rods glued to it, between which a slider, *D D*, may be moved from east to west; it is supported by two projecting parts, *C C*; the brass nut, *E*, through which the elevating screw passes in a direction from east to west, is fixed with screws to the slider; *F* is the screw head at the western extremity, which in this figure hides the screw; *G* is the suspension thread attached to the screw.

Fig. 2. represents a view from the south, of the same suspender, with the screw and thread. *A A* here, is the longitudinal section of the board fixed to the ceiling; *B B* is the rod glued to this board on the north side; *C C* the support of the slider; it is furnished at the edge with a scale, for the adjustment of the slider; *D D* the longitudinal view of the slider, to which the copper nuts *E* and *E′* are fixed with screws. Through these nuts passes the elevating screw, the head of which is represented by *F*. This screw passes through the nut *E*, and is kept in its place by the nut *H*. Near to the second nut *E′* the screw changes into a smooth cylinder which passes through a smooth aperture of the nut *E′*. At the end of the thread of the screw the suspension thread *G* is fastened, and lies in the grooves, in which it continues to the centre, and there falls perpendicularly, bearing at its lower end the stirrup of the magnetometer. When the thread is to be raised, the nut *H* is loosened, and the screw turned by the screw-head *F* into the required position.

Fig. 3. presents a view, from the west, of the vibrating portion of the magnetometer. It consists of two eyes, *A A*, of which the posterior is concealed in this figure by the anterior. The lower end of the thread *G* is fastened to a pin fixed under them. To this part of the magnetometer belongs also the torsion-circle *B B*, upon which the stirrup *C C C C* rests; the magnet bar *D D*, and the mirror-holder *E*, with two frames *F F*, *H H*, and the clamps *K K*, serving to receive the mirror. With the exception of the magnet bar, which alone weighs 1700 grammes, and of

the mirror, which must be of such thickness that it may not bend, all the other parts are constructed of thin brass, so as to increase the moment of inertia of the magnetometer as little as possible. The thread supporting the stirrup is not fastened immediately to it, but to a pin which fits below the staples *A A*, so that without unfastening it may be disengaged from the stirrup. The pin is provided with two small points, at a distance of about 40 millimetres from each other, which fit into two depressions on the staples *A A*. The torsion circle *B B* is furnished with a vertical pivot, the upper end of which supports the staples *A A*, and is surrounded by the rotating stirrup. The stirrup itself rests upon the periphery of the torsion circle, but is prevented from turning by its friction against it. At the end of the magnet bar *D D* is observed the mirror-holder, which at *E* forms a sheath incasing the magnet bar, to which it can be tightly fastened by screws. To this sheath is attached a frame *F F'* turning round a vertical axis. Small pressing and tightening screws, which serve for placing and fixing this frame, are behind it in this view, and therefore are not seen. With this first frame *E F'*, turning round a vertical axis, is connected a second frame *H H*, turning round a horizontal axis at *F'*, which can be adjusted to the first by means of the screws shown above. The clamps which are to receive the mirror are attached to this second frame. Three such clamps exist; but in this figure only two, *K* and *K'*, are visible, while the third is covered by the second at *K'*.

Fig. 4. Serves to give a more distinct view of all the parts of the mirror-holder, which, here seen from the south, are severally better seen than in the foregoing view from the west. Each part is designated by the same letter. The rectangle seen between *E* and *E''* is the transverse section of the *sheath* inclosing the magnet bar, to which it is firmly screwed. This case has on one of its sides two projections, *E' E'*, which form the vertical (horizontal in the figure) axis of the frame *F F F' F'*. Opposite, near *E''*, is a third projection, against which the screws act, which serve for placing and holding fast this first frame. A horizontal (in our figure vertical) axis is attached to this first frame at *F' F'*, around which the second frame *H H H H* can revolve. Opposite to this axis both frames have small projections, whose relative distance can be adjusted by pressing and tightening screws. Three small incisions are shown, *H H*, *H H*, *H H*, into which three small sliders can be inserted and fastened. This

arrangement serves the purpose of adjusting the space necessary for the reception of the mirror. These three small sliders terminate at their southern extremity, in three small vertical circular surfaces, on which the edges of the mirror are placed; while the head of a screw, whose grooves fit into the sliders beneath the edge of the mirror, press on its front surface. In this figure the sliders themselves are not seen, but merely the heads of the three screws, which fit into and conceal them.

After these explanations of the first figures, a few short remarks respecting the others will suffice.

Fig. 5. In this view of the stirrup, torsion-circle, magnet bar, and mirror-holder, seen from above, the torsion circle is more distinctly presented to view, as also the form of the stirrup. In the centre of the circle is also visible the end of the pivot passing through the alidade, and the double staple attached to it, with its two pivot holes. The brass pin, whose points fit into these holes, is removed, for the sake of perspicuity. In this figure, moreover, is seen how the mirror is fastened to the mirror-holder.

Fig. 6. In this figure, which has often been referred to previously, is chiefly seen in what manner the points of the pin, to which the suspension thread is fastened, fit into the holes of the staples, which latter are connected by a centre-piece provided with a square aperture in its own centre, into which the 4-sided pin of the torsion circle is inserted, and held fast by a screw. Since the stirrup, together with the magnet bar, must be raised when the latter has to be inverted for the purpose of finding its magnetic axis, the pin to which the thread is fastened would then fall out, but for a small spring beneath, which is visible in this figure, and which then retains the pin in its position. The wooden rod, above 700 millimetres in length, which in this figure is laid across the centre of the magnet bar, and serves for the support of two half-kilogrammes which are to increase the moment of inertia of the magnet bar, is furnished with 6 points, on which the two weights can be placed at different distances. The two central points are at a distance of 100, the next two at a distance of 400, and the ex'reme points at a distance of 700 millimetres from each other. The first and last are fixed; the two intermediate ones can be taken out and placed in other notches, situated at distances of from 50 to 50 millimetres asunder. The distances of all these points must be measured with microscopical accuracy.

Figs. 7, 8, and 9 represent the pin to which the thread is fastened, seen from one side, from above, and from below. The first view exhibits the two points with which this pin fits into the holes of the staples of the torsion circle, as also the spring which retains it when the stirrup is raised, and the thread loosened. The second view shows the narrow, round aperture through which the thread passes and is held together. The third view exhibits an oval aperture, which is bisected by a round transverse pin. The thread is wound round this latter, and drawn tight, after having been longitudinally drawn through a loop formed by its inferior extremity.

Fig. 10. gives a representation of the scale which is fixed below the theodolite, and the reflected image of which is observed with the theodolite telescope. By employing an astronomical telescope (which, with a similar object-glass, is preferable, for clearness and definition, to the terrestrial telescope) the scale is inverted, so that the figures stand above the divisions, while, in our figure, they are situated beneath them.

Expense of building and furnishing a Magnetic Observatory.

The expenses consist in the cost of the *building* and the *instruments*. That of the building is not everywhere the same; at Göttingen, it amounted to 798* dollars, Prussian currency. A part of the costs were occasioned by the exclusion of iron in the nails, locks, hinges, and fastenings of all kinds, all of which are of copper.

The costs of the instruments, as supplied by Meyerstein of Göttingen, who has hitherto made the greatest number of such instruments, are as follows:

	Dollars.
1. An 8-inch theodolite	150
2. A seconds clock	
3. A stand for the theodolite	7
4. A scale, with frame	1
5. The illuminating apparatus	11
6. The suspender, with slider and screw . .	8
7. The stirrup, with torsion circle	15
8. A 4-lb. principal bar, with its case; a 4-lb. auxiliary bar, and a 1-lb. quieting bar	7
9. A brass torsion bar, with magnets inlaid . .	9

* The Prussian current dollar is equal to three shillings.

	Dollars.
10. Two mirror-holders, with adjustments and mirrors	43
11. A weight-holder, with two half kilogrammes with hooks	7
12. A case with glass lid	16
13. Three measuring scales, 6 metres long . . .	4

WEBER.

II.

Method to be pursued during the terms of Observation.

The six appointed terms every year fall towards the end of the months January, March, May, July, September, and November; they commence at noon, Göttingen mean time, on the last Saturday in each of those months, and terminate at noon on the following day; the sub-terms, which have hitherto been added to the principal terms (from 8 to 10 in the evening on the Tuesday and Wednesday of the following week) will in future be discontinued. According to the rule, the position of the magnet needle is in each term determined every 5 minutes, so that one term affords 289 results. The clock is regulated, in Göttingen, previous to the commencement of each term, accurately to mean time. As a near coincidence in the time of the individual determinations at various stations is highly desirable, most of the observers at other places are accustomed to regulate their clocks also to Göttingen mean time. Where this cannot be done, it is recommended that such whole minutes be chosen for the moment of observation as approximate nearest to the times of observation at Göttingen. If, for instance, it had been found, previously to the commencement of the term, that the clock-time required for the observations was about 13′ 48″ in advance of Göttingen mean time, the needle should be observed at 0^h 14′, 0^h 19′, 0^h 24′, 0^h 29′, and so forth of the clock. In every case, it is best to choose the *full* minutes.

The position of the needle to be determined for any instant of time is not that position which the suspended magnet bar actually has at the instant, but that which it would have were its magnetic axis at that instant exactly in the magnetic meridian. This distinction was unnecessary, as long as such needles only were employed as were not susceptible of very great accuracy: with them it was only requisite that the needle at the time of observation should not be in perceptible vibration, and the observation might at once be made. The instruments now in use are

susceptible of, and require much greater exactness; and no such immediate determination is possible. It is not in our power so completely to quiet the needle of the magnetometer that it shall have no visible vibration; at least, it cannot be done with certainty, without some expense of time, and not for long; therefore, instead of an immediate observation, we must substitute such indirect modes of determination as do not require the entire absence of vibratory motion.

The most obvious method consists in observing the needle whilst in vibration; noting two successive extreme positions (a maximum and a minimum) on the scale, and taking a mean between them. This, in itself unexceptionable course, requires however some modification, if the vibrations are of considerable magnitude; and if the vibrations are small, is admissible only under limited conditions. In the first case, the progressive decrease of the arc of vibration in successive oscillations will not be insensible; consequently the deviation from the true meridian on the *maximum* side will be less than it was on the opposite side at the preceding *minimum*; and the mean of this *minimum* and its following *maximum* will be too small. From the same cause the mean of that *maximum* and the *minimum* that follows it will give too great a result. As the decrease of arc is nearly uniform for a few vibrations, the mean of two such means may be considered sufficiently exact, and may be taken to correspond to the instant of the second elongation. To express this by a formula, if a, b, c are the readings for three successive elongations (it is indifferent whether the first and third are *minima*, and the second a *maximum*, or the reverse) $\frac{1}{4}(a + 2\,b + c)$ represents the position of the magnetic meridian at the instant of the elongation b. With small vibrations this course is only admissible when the declination does not undergo sensible changes in a short time; and, in such case, the mean between *two* successive elongations may be taken as the true position of the needle corresponding to the middle time: but this proceeding may be wholly unavailable when the declination is subject to sudden and considerable changes.

These methods of determining the position of the magnetic meridian from observed elongations, have always this inconvenience, that the instant to which the result obtained corresponds may not be that for which the position is required; and, although

in the majority of cases this may not be very important, yet a preference is clearly due to a method which is free from this objection, and combines convenience, uniformity, and all desirable accuracy; it is accordingly the method adopted by those who take part in the *term observations*.

This method is founded on the principle, that the mean between two positions of the needle, which correspond exactly to two instants separated from each other by the time of one vibration, coincides with that position of the magnetic meridian which existed at the mean of these instants, in whatever parts of the vibrating period the instants might have fallen. This principle would be mathematically true, if, on the one hand, no external causes (such as the resistance of the air, etc.) occasioned the successive diminution of the arc of vibration; and if, on the other, any possible change in the situation of the magnetic meridian might be regarded as uniform for that short interval. The first circumstance has, however, no perceptible influence, if the method is applied when the vibrations are very small; and, in regard to the second, the changes of declination during so short an interval are generally of themselves hardly perceptible, and therefore we are the more justified in regarding such changes as at least uniform for the short intervals in question*.

Thus, therefore, the question is solved. In order to learn the position of the needle corresponding to the declination for the time T, it is only necessary, after the vibrations have been reduced by suitable means, to observe the actual positions for the times $T - \frac{1}{2}t$, and $T + \frac{1}{2}t$, and to take the mean; t signifying the time of a vibration. For greater accuracy and certainty, however, similar determinations, and of equal number, should be made, at equal intervals, a few moments before, and a few moments after T: this being done, in as far as the alteration of the declination may be considered uniform for the time in question, the mean of all these results will be the *final result* corresponding to the time T, and will deserve more confidence than the single determination for T itself.

The mode of performing this is very simple: if, for instance,

* At times (although very seldom) cases have actually occurred, where traces of acceleration, or retardation of the change, in so short a period, could be plainly demonstrated. This subject shall, at some future time, be more fully treated.

the final result is to rest on five partial results, we note the actual position of the needle for the six times:

$$T - \tfrac{5}{2}\,t,\; T - \tfrac{3}{2}\,t,\; T - \tfrac{1}{2}\,t,\; T + \tfrac{1}{2}\,t,\; T + \tfrac{3}{2}\,t,\; T + \tfrac{5}{2}\,t.$$

If the divisions of the scale noted are called a, b, c, d, e, f, then $\frac{1}{2}(a + b)$ will be the result for the time $T - 2\,t$; in the same manner $\frac{1}{2}(b + c)$, $\frac{1}{2}(c + d)$, $\frac{1}{2}(d + e)$, $\frac{1}{2}(e + f)$ for the times $T - t$, T, $T + t$, $T + 2\,t$; and the mean of these partial results, or the fifth part of their sum, is to be taken as the corrected final result for the time T.

The detail of the observations in Göttingen, on the 17th of August, 1836, for $15^h\ 30^m$ may serve as an example. The observer was Dr. Wappäus. 20^s was taken as the value of t.

$15^h\ 29^m\ 10^s$	865·2		
30	867·5	866·35	
50	866·3	866·85	
30 10	868·0	867·10	867·16
30	867·3	867·65	
50	868·5	867·90	

The first column contains the times of observation; the second the divisions of the scale noted; the third the means between each two successive notations; which are, consequently the partial results corresponding to $15^h\ 29'\ 20''$, $15^h\ 29'\ 40''$, $15^h\ 30'\ 0''$, $15^h\ 30'\ 20''$, $15^h\ 30'\ 40''$, and the final result to $15^h\ 30'\ 0''$. In this example the continual variation of the declination in the course of the observations is evident, and is also confirmed by the preceding and following results: which were

$15^h\ 25'\ 0''$.........862·82
15 35 0 ....872·32

For the short time which this method of observation requires, it more frequently happens that the declination is nearly stationary throughout: when it is so, the greater or less deviation of the partial results from the mean serves as a sort of measure of the greater or less confidence due to the observations; whether depending on the degree of skill and attention on the part of the observer, or the goodness of the apparatus, or on more or less favourable external circumstances.

The method described is that which is followed by most of the participators in the term-observations. It presupposes the knowledge of the time of vibration of the needle, which, as is well known, is dependent on the magnetism of the needle, and on the intensity of the horizontal portion of the terrestrial magnetic force; so that, strictly considered, it is not quite

the same at different periods. Instructions for the accurate determination of the time of vibration will be given subsequently; but a very accurate knowledge is not requisite for the present purpose; and not only may the small variations to which it is subject be neglected, but it is even allowable to substitute the next full second for the accurate value, in order that the instant at which the observer has to determine the point of the scale under the vertical line of the telescope, may correspond always to full seconds. This happens of itself when the approximate time of vibration is an *even* number of seconds; when it is an *odd* number, one of the three following means may be chosen.

I. We may still keep to the nearest even number; and we may adopt this course the more readily if the difference between this number and the true value does not exceed half a second. The greater the time of vibration, the more easily will the needle be kept in a nearly quiescent state. The needle in the magnetical observatory at Göttingen has, for instance, at present, a time of vibration of $20^{s}\cdot64$; now, although the number 21 is here the nearest, yet we may generally employ the more convenient number 20^{s}, as the arc of vibration seldom exceeds a few divisions of the scale: it can easily be demonstrated, that the error originating *thence* cannot surpass the twentieth part of the arc in a partial result, or the hundredth in a final result. On the other hand, to an observer whose needle has a time of vibration of $10^{s}\cdot64$, and especially if he has not a like perfect quiescence at his command, it is recommended that the number 11 should be chosen, and one of the following modifications adopted.

II. Choose the odd number; but the instants of observation, which, according to the above formula, would fall on half seconds, must be taken either all half a second later, or half a second sooner; which obviously makes only this difference—that the final results do not correspond to the full minutes of clock-time, but to a half second more or less.

III. If the final result is not, as above, based on an odd, but on an even number of partial results, the times of observation fall of themselves on full seconds, whether the next entire number taken for the true value be odd or even. If, for instance, the final result depend on *six* partial results, then the times of observation are

$$T - 3\,t,\ T - 2\,t,\ T - t,\ T,\ T + t,\ T + 2\,t,\ T + 3\,t.$$

This process, by which the influence of the neglected fraction of the time of vibration is still more completely eliminated in the final result, is particularly to be recommended to those observers who employ smaller apparatus, or needles of comparatively shorter time of vibration.

It may also be observed, that as the addition of a small weight increases the time of vibration of the needle, it is possible so to arrange the weight, and the spot on which it is to be placed, that the time may be brought extremely near to an entire number of seconds. This resource has been adopted by some observers who had it not in their power to preserve their needles from vibrating in rather large arcs. It is, however, an insufficient expedient; for, even admitting that the conditions of the theorem are thus fulfilled, it is not possible to determine the fraction of a division of the scale corresponding to a given second with nearly the same exactness when the needle moves rapidly, as when its motion has been rendered so slow that the change in a whole second is scarcely perceptible. The importance of sufficiently quieting the movements of the needle cannot be too strongly insisted on. It is necessary for this reason that the intervals between the observations should be sufficiently long to admit of this operation whenever it is required.

With the needle of the magnetical observatory at Göttingen the intervening time is, with the first method, $3^m\ 20^s$; with the second, $2^m\ 54^s$; in both cases sufficient for the above purpose to practised persons. Observers commonly employ this interval (as the necessity of rendering the needle quiescent but rarely occurs) in calculating the final result. Where, however, the needle has a much longer time of vibration, and, consequently, the interval between two series of observations is much shorter, a modification of the above method is preferable.

The modification consists in this; that the partial observations are not separated from one another by the time of an entire vibration, but by an aliquot part of one; i. e. a half, or a third. Besides the advantage of shortening the time required for the observations of each series, and of thus gaining a longer interval between two series, we avoid the tedium of being unemployed during the greater part of the time intervening between the partial observations. Practised observers, therefore, frequently prefer this modification even when the time of vibration is not very long. In our observatory several observers make their no-

tations in intervals of 10″ (half of 20″), and even of 7″ (third of 21″). Some examples will best illustrate what further remarks we may have to make.

Observation on the 17th August, 1836, for 10ʰ 20′. *By Prof. Ulrich.*

10ʰ 19′ 30″	869·9		
40	871·3	870·80	
50	871·7	871·05	
20 0	870·8	871·35	871·35
10	871.0	871·60	
20	872·4	871·95	
30	872.9		

The second column contains the several notations; the third, the partial results; 870·80 is the mean between the first and third notation, and therefore corresponds to 10ʰ 19′ 40″, and so forth. It is pleasing to perceive in this example, chosen from a time of rapid change in the declination, how a practised observer can recognize with certainty the changes occurring in 10 seconds.

Observation on the 25th March, 1837, at 0ʰ 5′. *By Dr. Goldschmidt.*

0ʰ 4′ 32″	847·3		
39	847·2	848·00	
46	847·8	848 05	
53	848·7	847 95	
5 0	848 9	847·85	847·91
7	848·1	847·90	
14	847 0	847·70	
21	846.9		
28	847·3		

The first partial result in this case is obtained from the combination of the first and fourth notations; the second from that of the second and fifth, &c.

In this example the submultiple of the approximate time of vibration is an integer number; where this is not the case, the time must be divided into unequal parts, which has, however, no disadvantage, provided such an arrangement is made, that the notations to be combined shall always have for the interval to which they correspond the same approximate value of the time of vibration, and that the time, and also its portions, shall be registered. Thus, for instance, the observations in the astronomical observatory, with a bar of 25 pounds in weight, having a time of vibration of 43ˢ·14. must be arranged according

to the following scheme,—taking the approximate value at 43^s, dividing it into four parts, and deriving the final result from five partial results.

$$\begin{array}{rrr|}
0^h & 4' & 17'' \\
 & & 28 \\
 & & 39 \\
 & & 49 \\
 & 5 & 0 \\
 & & 11 \\
 & & 22 \\
 & & 32 \\
 & & 43
\end{array}
\quad
\left.\begin{array}{rrr}
0^h & 4' & 38''\cdot5 \\
 & & 49\ \cdot5 \\
 & 5 & 0\ \cdot5 \\
 & & 10\ \cdot5 \\
 & & 21\ \cdot5
\end{array}\right\} 0^h\ 5'\ 0''\cdot1$$

The first column contains the times of notation; the second the times to which the partial results severally correspond: it is obviously unimportant that the final result, if accurately taken, falls at $0^h\ 5'\ 0''\cdot1$. If the final result is based on six partial results, then the following scheme is adopted:

$$\begin{array}{rrr|}
0^h & 4' & 12'' \\
 & & 22 \\
 & & 33 \\
 & & 44 \\
 & & 55 \\
 & 5 & 5 \\
 & & 16 \\
 & & 27 \\
 & & 38 \\
 & & 48
\end{array}
\quad
\left.\begin{array}{rrr}
0^h & 4' & 33''\cdot5 \\
 & & 43\ \cdot5 \\
 & & 54\ \cdot5 \\
 & 5 & 5\ \cdot5 \\
 & & 16\ \cdot5 \\
 & & 26\ \cdot5
\end{array}\right\} 0^h\ 5'\ 0''$$

The advantage of this modification in the mode of observing is most evident, when it is desired to follow the course of the magnetic declination more closely than at intervals of 5 minutes. These intervals, sufficient for the ordinary progress of the changes of declination, are in fact too large for the examination of the greater and more rapid changes; and it was in this view, and because shorter intervals could scarcely be generally adopted throughout the terms of 24 hours, that subordinate terms were added, each of two hours' duration, in which the observations were to be made at intervals of 3 minutes. As, however, the subordinate terms occasioned some difficulties, and, as they have hitherto brought to light but few phenomena of corresponding importance, it has been decided to discontinue them. The same object can be attained even more effectually in another manner. The rule of observing at every 5 minutes is retained; but if at any time rapid changes of declination occur, the obser-

vations are made at every 2½ minutes, as long as it may appear desirable to do so. An example is added:

$10^h\ 22'\ 0''$	875·0		
10	874·8	875·50	
20	876·0	875·95	
30	877·1	876·40	876·27 for $10^h\ 22'\ 30''$
40	876·8	876·60	
50	876·1	876·90	
23 0	877·1		

Observers in general are requested to pursue the course here pointed out whenever occasion may require it; and, in such case, it cannot be doubted that, whenever changes of such magnitude occur, a body of corresponding observations in close detail will be collected, and will furnish interesting conclusions respecting these remarkable phænomena.

If observers, instead of a clock beating seconds, are furnished with time-pieces marking other divisions of time, they must arrange their observations in an analogous manner, corresponding to the beats of the time-piece. The observations with a chronometer are more difficult than with a clock, particularly if the second hand is not truly centred, as is sometimes the case.

It may be well to add some general precautions for unpractised observers.

It is of the first importance that the movement of the needle should be perfectly free. Spiders sometimes get into the box, and attach their web to the needle. This may be so fine as possibly to escape observation with the eye. Previously to each term, therefore, the finger should be passed carefully round the needle on every side. Any impediment which may exist to free motion will diminish the time of vibration of the needle. The most minute spider's thread has a very considerable effect in this respect, of which a curious example will be related in its place.

In night observation it is necessary to illuminate the scale, which, at Göttingen, during the term-observations, is done by means of two Argand lamps. There is always an upward current of heated air above the flame, and, therefore, if one of the lamps is placed near and below the telescope, such a current passing before the object-glass will impair the distinctness of vision, and cause the divisions of the scale to appear tremulous and undulating. This inconvenience frequently occurred

at Göttingen in the first observations; but has completely ceased since each lamp has been provided with a copper chimney, directed to the side.

As in the term-observations several observers are required, there may be a considerable difference in the distance at which distinct vision is obtained by the several individuals. If a short-sighted person comes to the telescope adjusted for a long-sighted person, some alteration will be required for distinct vision. The use of a concave glass would be inconvenient and unadvisable, on account of the considerable loss of light. The mere sliding of the eye-tube is not sufficient, as, although the image of the scale might thereby be rendered distinct, the cross threads would remain indistinct, and would have a parallax in respect to the image of the object. It would be necessary, therefore, (with the construction which the telescopes employed in these observations usually have) that the cell containing the cross threads should be moveable in the eye-tube, and that it should be brought nearer to the lens in the eye-piece; but this requires a practised hand, takes time, and for other reasons is not to be recommended for the present case. The difficulty may, however, be got over in a very simple manner, if the following plan be adopted. The eye-tube in the telescope, and the cross threads in the same, are to be so adjusted previous to the observations, that the most short-sighted among the observers can see perfectly distinct both the image of the scale and the cross threads; when a longer-sighted person arises in turn, he has merely, without displacing the eye-tube or the cross threads, to draw out the glass nearest the eye so far that he can define perfectly well the cross threads, and with this a completely distinct vision of the image of the scale is necessarily connected. A short-sighted person coming in turn has merely to make an adjustment in the contrary way.

For the purpose of proving the undisturbed state of the telescope, a mark is employed, which is placed at such a distance that it may be seen distinctly with the same position of the eye-piece as is required for the distinct vision of the image of the scale; this consists, in the Göttingen observatory, of a small vertical line on the northern wall*. Previously to the commence-

* With respect to this arrangement, I may here observe that a mark for the verification alluded to must be considered as indispensable. Previously to the building of the present Göttingen magnetic observatory, it was doubted

ment of the observations, the telescope must be directed towards the mark, and this examination must be repeated from time to time; and if a deviation is indicated in the optical axis, it must be again brought back to its original vertical plane. If the precaution is taken to note two other divisions on the wall, one on either side of the mark, they will furnish the means of estimating the amount of the requisite correction. But it should be remembered that these divisions, though they may be made to correspond exactly with the divisions of the scale, will not have exactly the same value in seconds. If no such auxiliary marks have been made, the amount of the correction must be judged of by the eye, in parts of the divisions of the scale itself.

The observations are made at the vertical thread; the horizontal thread serving merely to indicate nearly the middle of the former. In order that it should make no difference whether the parts of the scale appear somewhat higher or lower in the field of view, the cross threads must have such a position, that a fixed object, seen on their crossing, remains accurately on the vertical thread, when the telescope is moved somewhat up and down. The mark also serves for this verification, which, however, need not be frequently repeated when the position is left unchanged.

The plumb-line suspended from the centre of the object-glass must be so near the scale that the image of both may appear with the same distinctness in the telescope, and that thus the division covered by the line may be precisely determined. The scale must be so placed that its zero must correspond with the plumb-line, or the division which does so correspond must be taken as an arbitrary zero. The verifying the undisturbed state of the scale should be repeated from time to time in the course of the

whether it was not better to place this mark on an insulated pedestal in the interior of the room, than on an exterior wall exposed to the weather. The latter was decided on, as otherwise either the distance of the observer from the needle must have been diminished,—or the advantage of seeing distinctly the mark and the scale with the same position of the eye-piece be given up,—or the room must have been made of a greater length, which was not possible in the place fixed on. To have a separate foundation for a mark was regarded for many reasons as objectionable. Moreover, the fear that the place of the mark might be perceptibly altered by the influence of the weather on the wall, was regarded as of little importance, considering the solid construction of the building, and the small height of the mark above the foundation; and especially as it was in our power to repeat, as frequently as desired, the measurement of the angle between the mark and a church spire seen through the northern window. The experience of three years justifies the propriety of this arrangement.

observations; it is, however, not requisite, when a small change is found, to bring back the scale to its former position; it is sufficient to note down in the registry the point of division corresponding to the plumb-line.

It may probably not be superfluous to draw attention to one or two points of comparatively minor importance. It has been supposed, that the magnetometer and telescope are so arranged that the mean position of the magnetic declination corresponds to about the centre of the scale. However, at times of considerable variation, this centre frequently gets entirely out of the field of view, and then the above method of verification will no longer answer. If at such a time the verification appears necessary, the quieting bar must be made to perform an exactly opposite office to that which it generally serves; namely, to give the magnetometer a vibration of sufficient extent to reach, and even to go rather beyond, the spot required, and thus to allow the plumb-line to appear in the middle of the field, at that part of the vibration where the motion is slow, and where consequently the corresponding division of the scale can be determined with accuracy. It is obvious that if such cases occur in the course of a periodical series, the magnetometer must be again quieted in time for the next observation, and, consequently, skill in the use of the quieting bar is of great moment.

When the declination falls very nearly in the centre of the scale, unpractised observers must be on their guard not to confound the plumb-line with the vertical line of the telescope. In our apparatus both resemble one another so much, that with a very quiet state of the needle, a mistake is very possible, and did, indeed, once occur. When there is danger of such a mistake, it may be expedient temporarily to remove the plumb-line.

With respect to the form of communication, some persons are accustomed to send in the observations *in full*, others the *partial* and *final results* only, and several merely the latter. The last may be sufficient, if the calculations have been revised, and the communicated numbers collated; but the observations themselves should be preserved, in case a reference should be wished; and when unusually great changes occur, communication, in full detail, is most desirable. Besides the results of the observations, it is always proper to notice, in connection, the value of the parts of the scale (or the measurements on which the determination is founded), the time of vibration, the correction and rate of the

clock, the name of the observer, and remarks on such observations as may be somewhat doubtful. An early communication is always greatly to be desired. GAUSS.

III.

Extract from the daily Observations of Magnetic Declination during three years at Göttingen.

To discriminate the *regular* changes of declination, amidst those incessant changes of greater or less amount, which we call *irregular*, in so far as their occurrence seems unconnected with any periodical rules, requires a great number of observations on a fixed plan, persevered in for a length of time, in order to deduce, by suitable combinations, mean values, freed as far as possible from the influence of those anomalies by which the individual declinations are affected. In general, in this part of the globe, the declination increases during the forenoon, but the increase is unequal on different days; it even sometimes happens, though rarely, that at the usual hour of maximum, the declination is less than it was during the earlier part of the same day. The cause of the morning increase may be in operation every day; but its influence is sometimes increased, sometimes diminished, and sometimes entirely masked, by other irregular intervening forces. Observations on a single day, or continued for a few days only, cannot therefore determine either the amount of the effect due to the regular cause, or its inequalities at different seasons. For this, mean values, taken from a great number of days, are required. The same is the case with those progressive changes which take place in one direction for a very long time; these we call secular, because they require a long series of years to amount to many degrees. Single observations, repeated after an interval of only a few years, even though performed on the same day, in the same month, and at the same hour, can afford us no certain knowledge respecting them; but mean numbers, obtained by continued observations, allow us to anticipate, at the end of very few years, what it would otherwise take many tens of years to fix with any considerable degree of approximation.

With this view, from the very commencement of the observations to be performed at our Magnetic Observatory, I have included among them the daily determination of the absolute de-

clination at the same hour. In order to be able to calculate more easily on the possibility of a long and continuous perseverance, by which alone labours of this kind can be of value, I have at first rather chosen a limited plan than attempted to combine too much at once. On this account only two observations are made daily; at eight in the forenoon, and one in the afternoon, according to mean time. These hours, which were most easily compatible with other duties, are also suitable ones, because in the regular course of the magnetic movements the position of the needle at 1, P.M. is never far from the maximum of declination, and during the greatest part of the year, the hour of minimum is not far from 8, A.M. Observations at fixed hours of apparent solar time would, it is true, have been more in accordance with nature; but the much greater facility of an arrangement made according to mean time, renders it deserving of preference in this case, where the chief point is to secure a persevering continuance in one and the same principle.

A regular register was commenced on the 1st of January, 1834; but the first two months and a half have been omitted in the following extract, because during that time it was frequently necessary to wind up the suspension-thread, whereby changes were produced in the torsion which were at first not sufficiently attended to. From the 17th of March a stronger suspension-thread was employed, consisting of 200 fibres, of which the point of no-torsion had been previously accurately determined; whenever changes were subsequently made in respect to the thread, or to any other circumstance connected with the elements of reduction, the necessary corrections, or modifications of those elements, have each time been applied. During the first months various sufficiently practised observers took part with me in the observations; but since the 1st of October, 1834, they have been regularly made by Dr. Goldschmidt, his place having been only occasionally supplied, when necessary, by other expert observers.

I have already communicated in the Göttingen *Gelehrten Anzeigen*, 1834, p. 1269, and 1835, p. 345, the monthly means deduced from these determinations up to January, 1835: they are now given for three entire years.

Mean value of the Westerly Magnetic Declination at Göttingen.

	8, A.M.	1, P.M.
1834. March, second half.	18° 38′ 16″·0	18° 46′ 40″·4
April	36 6·9	47 3·8
May	36 28·2	47 15·4
June	37 40·7	47 59·5
July	37 57·5	48 19·0
August	38 48·1	49 11·0
September	36 58·4	46 32·3
October	37 18·4	44 47·2
November	37 38·4	43 4·3
December	37 54·8	41 32·7
1835. January	37 51·5	42 14·4
February	37 3·5	42 29·4
March	34 47·5	44 55·2
April	32 57·7	46 31·6
May	32 13·4	45 17·1
June	32 56·4	44 41·3
July	34 8·0	44 42·8
August	34 12·4	46 56·8
September	33 21·2	44 27·6
October	33 23·0	43 5·3
November	36 15·3	43 49·5
December	35 25·9	40 19·1
1836. January	35 2·4	40 34·6
February	33 26·7	41 15·2
March	31 1·4	43 16·4
April	26 32·9	43 42·6
May	28 0·8	44 37·2
June	27 35·1	42 52·4
July	26 54·2	42 26·0
August	25 42·4	41 45·0
September	26 14·6	40 59·6
October	27 34·0	40 32·8
November	29 21·0	36 54·3
December	29 13·7	35 46·8
1837. January	27 35·3	37 46·2
February	27 35·6	36 28·3
March	25 44·2	39 4·2

Some combinations of these observations may now be noticed.

The difference between the declination of the morning and afternoon has one sign all through in the monthly means; the dependence of its magnitude on the season of the year will be perceived in the following tabular view:

	1834—1835.	1835—1836.	1836—1837.	Mean.
	′ ″	′ ″	′ ″	′ ″
April	10 56·9	13 33·9	17 9·7	13 53·5
May	10 47·2	13 3·7	16 36·4	13 29·1
June	10 18·8	11 44·9	15 17·3	12 27·0
July	10 21·5	10 34·8	15 31·8	12 9·4
August	10 22·9	12 44·4	16 2·6	13 3·3
September ...	9 33·9	11 6·4	14 45·0	11 48·4
October......	7 28·8	9 42·3	12 58·8	10 3·3
November ..	5 25·9	7 34·2	7 33·3	6 51·1
December....	3 37·9	4 53·2	6 33·1	5 1·4
January	4 22·9	5 32·2	10 10·9	6 42·0
February	5 25·9	7 48·5	8 52·7	7 22·4
March	10 7·7	12 15·0	13 20·0	11 54·2
Mean....	8 14·2	10 2·8	12 54·3	10 23·8

It will be perceived that, not only in the mean values, but also in each of the separate years, the difference has been smallest in December; and this is what we might expect, as those changes which vary according to the time of the day must necessarily be ascribed to the action of the sun, although as yet we know not *how* this action is effected. It may at first appear surprising, on the other hand, that the differences are not greatest at the time of the summer solstice, but appear smaller in June and July than in April, May, and August, especially as the coincidence of all three years in this circumstance affords a presumption that it is not accidental. It must not, however, be overlooked, that in the months immediately following the solstice, the time of the minimum of the declination is earlier, and therefore the whole increase would be sensibly greater than the change reckoned from 8 o'clock.

It is further observable that in each month the differences are greater in the second year than in the first; and again, in the third year greater than in the second. But these differences are by far too great in amount to admit of our considering them as parts of a secular increase, and it is rather to be expected that by continuing the observations for several years we shall not fail to discover a fluctuation. But, in any case, we hereby learn that one year may differ from another in respect to the effect of the sun on the earth's magnetism, somewhat in the same way that one summer or one winter differs from another in temperature. On this account also we shall only arrive at an accurate determination of the mean values by observations continued for several years.

It has been already stated that exceptions sometimes occur on single days, when the difference between the forenoon and afternoon declinations may have the opposite sign. But such exceptions are rare; during the three years' observations only fourteen cases of the kind have occurred; or, on an average, one in 79 days. I give them in this place, together with the amount by which, on each occasion, the declination at 8, A.M. exceeded that at 1, P.M.

	′ ″		′ ″
1834. Aug. 15	6 8·0	1835. Nov. 8	3 42·2
Dec. 24	3 43·0	Dec. 8	18 35·6
Dec. 25	0 38·2	1836. Jan. 20	0 46·3
Dec. 26	2 20·3	July 20	5 8·8
1835. Jan. 30	0 23·8	Nov. 9	11 9·5
Feb. 7	0 32·5	1837. Feb. 13	4 1·0
Oct. 4	0 43·1	Mar. 14	1 22·6

Of these fourteen exceptions, twelve, as might be expected, occur in the winter months, and only two in the summer months; the small regular action of the sun in the former being more easily exceeded by an anomalous movement than could be the case in regard to the far greater regular action in the summer months.

To try how far the secular variation might be recognised in the present observations, the monthly means of the first year have been compared with the corresponding ones of the second, and these with those of the third year. Among the forty-eight comparisons thus obtained (for the incomplete month of March, 1834, has been excluded from this as well as from all the other combinations), forty-seven give a decrease, and only one an increase, which is therefore characterised in the following table by the sign —.

Yearly Decrease of the Declination.

	First Year.		Second Year.		Mean.
	8, A.M.	1, P.M.	8, A.M.	1, P.M.	
	′ ″	′ ″	′ ″	′ ″	′ ″
April	3 9·2	0 32·2	6 24·8	2 49·0	3 13·8
May	4 14·8	1 58·3	4 12·6	0 39·9	2 46·4
June	4 44·3	3 18·2	5 21·3	1 48·9	3 48·1
July	3 49·5	3 36·2	7 13·8	2 16·8	4 14·1
August....	4 35·7	2 14·2	8 30·0	5 11·8	5 7·9
September .	3 37·2	2 4·7	7 6·6	3 28·0	4 4·1
October....	3 55·4	1 41·9	5 49·0	2 32·5	3 29·6
November .	1 23·1	— 0 45·2	6 54·3	6 55·2	3 36·8
December..	2 28·9	1 13·6	6 12·2	4 32·3	3 36·7
January ...	2 49·1	1 39·8	7 27·1	2 48·4	3 41·1
February ..	3 36·8	1 14·2	5 55·1	4 46·9	3 52·2
March	3 46·1	1 38·8	5 17·2	4 12·2	3 46·6
Mean...	3 30·8	1 42·2	6 21·7	3 30·2	3 46·2

That the comparison of the forenoon means should give in general a greater decrease than the comparison of those of the afternoon, is only a consequence of what has been stated above, viz. that the diurnal movements in the first year are smaller than in the second, and those of the second smaller than in the third. That difference must, therefore, not be considered as a real one, but merely as accidental; and we may expect, by a longer continuation of the observations, a difference in the opposite direction. As then there is no sufficient reason for preferring one of the results to the other, we can only take the mean of the two. The mean is, in the first year, 2′ 36″·5; in the second, 4′ 55″·9. We might regard this as a proof of an increasing rate of diminution in the declination: but this would be nothing more than a bad reason for a thing which in itself is true. It is known that the declination, which increased through all Europe during the last century, attained its maximum in the present century, and is now decreasing afresh. This transition must necessarily produce at first an imperceptible, and gradually a greater decrease. But although, from want of earlier observations, the precise year cannot be fixed in which the transition took place at Göttingen, yet it must be inferred from the observations made at other places, that it must have been at an earlier period than would follow from those two numbers, if we were to consider them as pure effects of the slow movement which we term secular. All other experience shows that 2′ 19″·4 is too great to be looked upon as a regular increase for a year. We, therefore, regard this difference as being for the most part accidental; so that, for the present, and until we have further experience to guide us, we must consider the mean value, 3′ 46″·2, as the annual decrease of the declination from 1834 to 1837.

As the difference between the declination in the forenoon and afternoon is subject to an inequality, evidently varying with the season of the year, the question arises whether the change depending on the period of the year affects one of these declinations only, or one more than the other, or both equally, and what are the existing laws in this respect. A longer series of years, it is true, will be required to find out this law, than is needed to determine the mere difference between the declinations; nevertheless, it will be desirable to see what light the present observations throw on the subject.

With this view the mean values for every twelve months have in the first place been calculated. They are:

	8, A.M.	1, P.M.
1834—1835	18° 37′ 12″·5	18° 45′ 27″·0
1835—1836	33 42·0	43 44·8
1836—1837	27 20·3	40 14·6

These mean values correspond to the middle day of each year of observation, *i. e.* to the 1st of October 1834; and so forth.

The comparison of the separate months of each year with the corresponding mean value, gives the following differences:

Declination, 8, A.M.

	First Year.	Second Year.	Third Year.	Mean.
April	− 1′ 5″·9	− 0′ 44″·3	− 0′ 47″·4	− 0′ 52″·5
May	− 0 44·6	− 1 28·6	+ 0 40·5	− 0 30·9
June	+ 0 27·9	− 0 45·6	+ 0 14·8	− 0 1·0
July	+ 0 44·7	+ 0 26·0	− 0 26·1	+ 0 14·9
August	+ 1 35·3	+ 0 30·4	− 1 37·9	+ 0 9·3
September	− 0 14·4	− 0 20·8	− 1 5·7	− 0 33·6
October........	+ 0 5·6	− 0 19·0	+ 0 13·7	− 0 0·1
November	+ 0 25·6	+ 2 33·3	+ 2 0·7	+ 1 39·9
December......	+ 0 42·0	+ 1 43·9	+ 1 53·4	+ 1 26·4
January	+ 0 38·7	+ 1 20·4	+ 0 15·0	+ 0 44·7
February	− 0 9·3	− 0 15·3	+ 0 15·3	− 0 3·1
March	− 2 25·3	− 2 40·6	− 1 36·1	− 2 14·0

Declination, 1, P.M.

	First Year.	Second Year.	Third Year.	Mean.
April	+ 1′ 36″·8	+ 2′ 46″·8	+ 3′ 28″·0	+ 2′ 37″·2
May	+ 1 48·4	+ 1 32·3	+ 4 22·6	+ 2 34·4
June	+ 2 32·5	+ 0 56·5	+ 2 37·8	+ 2 2·3
July	+ 2 52·0	+ 0 58·0	+ 2 11·4	+ 2 0·5
August	+ 3 44·0	+ 3 12·0	+ 1 30·4	+ 2 48·8
September	+ 1 5·3	+ 0 42·8	+ 0 45·0	+ 0 51·0
October.... ...	− 0 39·8	− 0 39·5	+ 0 18·2	− 0 20·4
November	− 2 22·7	+ 0 4·7	− 3 20·3	− 1 52·8
December......	− 3 54·3	− 3 25·7	− 4 27·8	− 3 55·9
January........	− 3 12·6	− 3 10·2	− 2 28·4	− 2 57·1
February	− 2 57·6	− 2 29·6	− 3 46·3	− 3 4·3
March	− 0 31·8	− 0 28·4	− 1 10·4	− 0 43·5

The numbers in the last column, being mean values deduced from three years' observations, are freed in some degree, though

still but imperfectly, from the influence of the irregular anomalies; but they are evidently still charged with the secular change. In order to eliminate this, its amount between the middle of each month and the 1st of October must be applied with the negative sign for the first six months, and with the positive sign for the last six months. Assuming the above determined value for the twelve months, 3′ 46″·2, we obtain the following results.

	8, A.M.	1, P.M.	Mean.
	′ ″	′ ″	′ ″
April	− 2 35·6	+ 0 54·2	− 0 50·7
May	− 1 55·3	+ 1 10·0	− 0 22·6
June	− 1 6·6	+ 0 56·7	− 0 4·9
July	− 0 32·0	+ 1 13·6	+ 0 20·8
August	− 0 18·8	+ 2 20·7	+ 1 0·9
September	− 0 43·0	+ 0 41·6	− 0 0·7
October	+ 0 9·3	− 0 11·0	− 0 0·8
November	+ 2 8·0	− 1 24·7	+ 0 21·6
December	+ 2 13·3	− 3 9·0	− 0 27·8
January	+ 1 50·3	− 1 51·5	− 0 0·6
February	+ 1 21·3	− 1 40·1	− 0 9·4
March	− 0 30·9	+ 0 59·6	+ 0 14·3

In these results we find as much regularity as could be expected from the observations of three years only. The first column shows how much the forenoon declination in each month differs from the mean forenoon declination, and in the same way the second column shows the difference of the afternoon declination in each month and the mean afternoon declination. It must be recollected that the latter mean itself is 10′ 23″·8 greater than the former mean.

It appears remarkable that in all the twelve months the forenoon and afternoon declinations fluctuate about their mean values in *opposite directions*. In the five winter months, from October to February, the forenoon declination is greater, and the afternoon declination less, than their respective mean values; and *both* circumstances tend, during this portion of the year, to render the differences less than their mean amount. In the other seven months exactly the opposite effect takes place. Moreover, these opposite fluctuations are, upon an average, nearly of equal magnitude; the consequence of which is, that they nearly destroy each other in the mean given in the last column. This result may be thus stated in other words: the mean

between the magnetic declinations of 8 A.M. and 1 P.M. does not contain (apart from the irregular anomalies and the secular decrease) any very important fluctuations dependent on season; at least there is no certain indication of any such difference between the summer and winter months.

The mean declination itself, deduced from the observations of the three years, is 18° 37′ 56″·9 for the 1st of October, 1835; meaning, of course, thereby, only the mean value of the hours chosen for our observations, from which the mean value of all hours of the day may perhaps differ a little, though probably but little. But all our previous researches abundantly show that without very long and wearisome labour nothing can be fixed with certainty respecting this.

Hitherto we have spoken only of monthly mean numbers. The complete publication of the separate observations would for the present be regarded as superfluous, since, being confined to one place, they present no interest but that arising from the irregular fluctuations which they display. This end may however be attained in a better manner than by the mere view of the numbers, by a methodical combination, in which the amount of the fluctuations is reduced to a definite measure, and the general character of the periods, in respect to the magnitude of the fluctuations during them, may be accurately compared. For the sake of precision, I understand here by fluctuation of the magnetic declination, the difference from that of the preceding day at the same hour; and, (according to analogy with what are called mean errors of observation,) I understand by mean fluctuation, during any given interval of time, the square root of the mean of the squares of the several fluctuations. It must here be remarked, that when several equal intervals, or intervals considered as equal, are united in one, the arithmetical mean of the partial mean fluctuations must not be taken as the general mean, but we must revert to the squares, and take the square root of their arithmetical mean. The results of the three years' observations, calculated in this manner, and expressed in seconds, are contained in the following table:

Mean Fluctuation of the Magnetic Declination during the three years from 1834 *to* 1837.

	8, A.M.			Mean.	1, P.M.			Mean.
	I.	II.	III.		I.	II.	III.	
April	74	126	205	147	129	101	264	180
May	192	124	277	207	158	183	210	185
June	172	171	199	181	95	151	217	162
July	213	243	287	250	119	184	252	193
August	264	253	269	262	175	165	307	225
September	162	325	207	241	172	143	161	159
October........	116	296	216	222	182	202	242	210
November	79	205	308	218	170	173	126	158
December......	132	324	71	206	184	206	154	182
January	146	274	138	196	174	212	154	181
February	116	146	164	143	178	183	129	165
March	100	109	366	228	127	153	246	183
Mean...	157	229	238	211	156	174	213	183

In reference to the single observations, we may here mention the *greatest* fluctuations which have occurred in the course of three years, in the forenoon and afternoon declinations. The greatest forenoon fluctuation was on the 8th of October, 1835, when the declination was about 20′ 1″ greater than on the 7th of October; and the greatest afternoon fluctuation occurred on the 24th of April, 1836, when the declination was 13′ 0″ greater than on the preceding day. On the other hand, perfect equality either of the forenoon or of the afternoon declinations, for two successive days, is a circumstance which has frequently occurred. In the monthly mean fluctuations these extremes naturally come much closer together; nevertheless, the great dissimilarity of the single months in this respect is still very remarkable, since, according to the above general view, the mean fluctuation in the forenoon declination in March, 1837, amounted to 6′ 6″; in December, 1836, only to 1′ 11″.

Whether, in general, greater fluctuations prevail at one time of the day than at others, cannot be determined with certainty, from the results of our observations of 8 A.M. and 1 P.M. Both are nearly equal in mean fluctuation for the first year; in the two following years the forenoon fluctuations exceed in amount, but the difference in the final results from the three years, 3′ 31″ and 3′ 3″, is too small to allow of any conclusion being yet esta-

blished, although, in the mean numbers for the single months, in the fourth and eighth columns, ten months give a difference in the same direction.

By combining the forenoon and afternoon fluctuations we obtain the following means:

	First Year.	Second Year.	Third Year.	Mean.
April	108	114	237	164
May	176	156	245	196
June	139	161	208	172
July	173	215	270	223
August	224	214	289	244
September	167	251	185	204
October........	152	254	229	216
November	133	190	235	191
December......	160	271	120	195
January	160	245	146	189
February	150	166	148	155
March	114	133	312	206

Mean Values.

	First Year.	Second Year.	Third Year.	Mean.
July to December	170	234	228	213
The remaining months..	143	167	223	181
Entire year	158	204	226	198

According to the numbers of the fourth column somewhat greater fluctuations occur in the months from July to December than in the other six; but the mean values 3′ 33″ and 3′ 1″ have too small a difference to justify a conclusion that greater fluctuations commonly prevail in that period of the year, especially as the difference has been principally occasioned by an excess in the single year 1835–1836.

On the other hand, the inequality of the fluctuations in each of the three years, in relation to one another, is very perceptible; the mean value for the third year being about half as large again as that of the first year. The general mean, from all observations hitherto made, 3′ 18″, might therefore be considerably changed by a longer continuance of the observations.

These are the results which may be drawn from the daily register kept hitherto. It is highly desirable that similar observations should be made at several stations, and at some they have recently commenced. If, as is done at Milan, the observations

were made, not according to the time of the place, but simultaneous with those of Göttingen, the comparison of the single days would afford an opportunity for other combinations, which, when continued through a considerable time, would possess great interest. Observers who follow this plan, *i. e.*, of making the times of observation simultaneous with ours, are requested to communicate the daily observations; this may be done in divisions of the scale, provided the necessary elements of reduction are at the same time communicated. GAUSS.

IV.

Description of a small portable Apparatus for measuring the absolute intensity of Terrestrial Magnetism.

Among the numerous applications of the magnetometer, the most important is that of measuring the absolute intensity of the earth's magnetic force, as described in the memoir entitled, *Intensitas vis magneticæ terrestris ad mensuram absolutam revocata; Auctore Carolo Friderico Gauss*: Göttingen, 1833. Frequent mention will be made in the course of this work of this application of the magnetometer, which enables us to compare numerically with one another the results of experiments made in the most distant parts of the globe, at different epochs, and with apparatus not previously compared. Everything necessary to be known for these experiments, as well as everything that may serve to facilitate them, will be communicated from time to time. Results of such absolute measurements will also be noticed, and their value shown in establishing, on a scientific basis, the science of galvanism.

These important absolute measurements can be performed with the accuracy they deserve only with the magnetometer, and, indeed, only in a completely furnished observatory. Few such observatories, however, exist at present, and few philosophers, therefore, have these means at their disposal; while there are many who take an interest in the results, and desire to be enabled to form such an opinion concerning them as can hardly be satisfactorily obtained without actually taking part in the observations and calculations, even though less minute and accurate. The simple means which it is the object of this chapter to describe may be procured by every person. The description and mode of employing them are with the more propriety given

here, because these pages are intended not merely for the limited number of those who participate in the simultaneous observations, but for all who are engaged in investigating the laws of magnetic phenomena.

Those less delicate instruments which were employed for magnetic measurements before the invention of the magnetometer, may not only be used for the same purposes as formerly, but may also be applied to those absolute measurements of intensity which owe their origin to the invention of the magnetometer. It is true that these instruments are far from affording such accurate results as the magnetometer; but the results they give are more easily obtained. On this account they have not lost all their value by the later invention; they may still be usefully employed, though in a more limited sphere. Wherever, from want of means or time, or from any other circumstances, magnetometers cannot be employed, these instruments may still be used with advantage. This will be the case most frequently in voyages and journeys to remote parts of the world. It is true that magnetometers *may* be carried on journeys, as was done by M. von Waltershausen and Dr. Listing in their Italian tour; but this is only possible for those who are highly favoured by external circumstances; and it is therefore not to be expected that many will follow this praiseworthy example. If, therefore, we wish to collect observations from the whole surface of the earth, we must be content with such as are not made with magnetometers; and it is important to extend the application of portable instruments to the absolute measurement of the intensity, which has been hitherto performed with the magnetometer only. The difference in respect to accuracy between the absolute measures with such instruments and those made with the magnetometer, is nearly the same as between measurements of declination with the two kinds of instruments. A skilful hand will be able to obtain useful results even with the smaller apparatus; and it appears desirable, therefore, that it should be extensively employed.

We shall consider successively,

1. The parts of the small apparatus.
2. The observations to be made with it.
3. The application of the observations.
4. The calculations required.
5. The result of the calculation.

6. The advantages, in point of accuracy, of the dimensions adopted in the apparatus.

1. *The parts of the small apparatus.*

In addition to a clock or chronometer, this apparatus consists of three parts:

1. A small compass needle.
2. A small magnet bar, which may be suspended to a silk thread, and vibrated.
3. A measuring scale 1 metre in length.

The needle of the compass from which the present description is taken was 60 millimetres in length, and the arc was divided to whole degrees only. In order that so small a compass should lead to useful results, it is necessary that the observer should be able to estimate with certainty the 10th part of a degree*. The needle may be somewhat larger; but the reasons, which render it advisable that it should never exceed 100 millimetres, will be given at the conclusion of this chapter.

The small magnet bar was 101 millimetres in length, 17½ in breadth, and 142 grammes in weight; it may be vibrated by suspending it to a silk thread bound crossways round the middle of the bar. It is advantageous that it should be made an exact parallelopiped, in order that, its weight and dimensions being known, its moment of inertia may be calculated. It may also be provided at the middle with a small hole, through which a sewing needle may be passed, in which case it is merely necessary to draw the suspension thread through the eye of the needle; it is better to make the small bar precisely 100 millimetres in length.

The breadth of the measuring scale must be such as to allow of the compass being placed on its centre; this scale need only be divided to 50 millimetres.

This simple apparatus is sufficient for the absolute measure of the magnetic intensity. It is furnished by M. Meyerstein, of Göttingen, for 9 dollars and a half, (of course, exclusive of the

* This estimation, which under other circumstances is easy to accomplish, presents in this case some difficulty, arising from the point of the magnetic needle being usually at a little distance from the divided arc: to get over this difficulty the following method is adopted: a mirror is laid horizontally on the table before the needle, and the eye, before it reads off the position of the needle, must be so placed that the prolongation of the needle would bisect the reflected image of the eye.

time-piece); so that this mode of measuring the intensity requires less expense than any other magnetic determination. It is also very portable and convenient for travellers. The apparatus is to be placed on a table in the middle of a room, avoiding all iron in the neighbourhood; large iron rails, even at some distance, must be carefully avoided. Arrangements may also easily be made for employing it in the open air.

2. *Observations to be made with this apparatus.*

These are of two kinds: 1. The experiments of deflection. 2. The experiments of vibration.

1. *The experiments of deflection.*

The measuring scale is placed horizontally, and at right angles to the magnetic meridian, with its zero point towards the east, and the needle in the centre. The small magnet bar is to be placed successively as follows:

1. With its north end to the east, on the zero point of the scale: if the length of the small magnet bar is 100 millimetres, its centre will then be over the division 50^{mm} of the scale. The needle will be deflected towards the east, and its position, u_0, is observed.

2. The south end is then substituted for the north end. The needle is deflected to the west, and its position, u_0', observed.

3. The north end of the magnet bar is placed towards the east, on the division 100 millimetres. The needle is deflected easterly, and its position, u_1, observed.

4. The bar is again reversed, end for end. The needle is deflected westerly, and its position, u_1', observed.

5. The north end of the magnet bar is placed towards the east, upon the division 150 millimetres. The needle is deflected easterly, and its position, u_2, observed.

6. The bar is again reversed, and the needle deflected westerly, and its position, u_2', observed.

7. The north end of the magnet bar is placed towards the east, upon the division 750 millimetres. The needle is deflected easterly, and its position, u_2'', observed.

8. The bar is reversed. The needle is deflected westerly, and its position, u_2''', observed.

9. The north end of the magnet is placed towards the east, on the division 800 millimetres. The needle is deflected easterly, and its position, u_1'', observed.

10. The bar is reversed. The needle is deflected westerly, and its position, u_1''', observed.

11. The north end of the magnet bar is placed towards the east, on the division 900 millimetres. The needle is deflected easterly, and its position, u_0'', observed.

12. The bar is reversed. The needle is deflected westerly, and its position, u_0''', observed.

These twelve observations may all be completed in half an hour.

2. *Experiments of vibration.*

The small magnet bar is to be suspended horizontally by a silk thread, to be set in vibration, and its time of vibration observed in the usual manner, which needs no further description here. The time of vibration may be determined by these experiments, with sufficient precision, in a quarter of an hour.

Taking together all the observations which are necessary for a complete measure of the absolute intensity, and allowing a quarter of an hour for arranging the apparatus and suspending the magnet bar, the experimental part of the determination can be completed in one hour. The observer may give his determination greater certainty and accuracy by repetition.

The following observations made with this instrument at Göttingen are given as an example.

Göttingen, January 18, 1837.

1. *Experiments of deflection.*

$$\begin{array}{ll}
1. & u_0 - u_0' = 23^\circ\ \ 9' \\
2. & u_1 - u_1' = 47^\circ\ 42' \\
3. & u_2 - u_2' = 71^\circ\ 48' \\
4. & u_2'' - u_2''' = 69^\circ\ 21' \\
5. & u_1'' - u_1''' = 46^\circ\ 12' \\
6. & u_0'' - u_0''' = 22^\circ\ 27'
\end{array}$$

In these experiments, the distance, R, of the centre of the small magnet bar from the centre of the compass, was successively,

$$\begin{array}{ll}
1. & R_0 = 450^{\mathrm{mm}} \\
2. & R_1 = 350 \\
3. & R_2 = 300 \\
4. & R_2 = 300 \\
5. & R_1 = 350 \\
6. & R_0 = 450
\end{array}$$

2. *Experiments of vibration.*

No.	Clock Time.	Number of Vibrations.	Their Interval.
0	0′ 3″25		
1	9·90	1	6″65
2	16·65	2	13·40
3	23·35	3	20·10
4	30·00	4	26·75
5	36·65	5	33·40
6	43·30	6	40·05
7	50·00	7	46·75
8	56·70	8	53·45
9	1′ 3·30	9	60·05
10	9·80	10	66·55
11	16·55	11	73·30
12	23·30	12	80·05
13	29·90	13	86·65
14	36·65	14	93·40
15	43·15	15	99·90
16	49·80	16	106·55
17	56·65	17	113·40
18	2′ 3·25	18	120·00
19	9·95	19	126·70
20	16·70	20	133·45
21	23·35	21	140·10
22	30·00	22	146·75
		253	1687″·40

Consequently the time of one vibration $t = 6''\cdot 67$.

3. *Application of the observations.*

A general and intelligible view of the application of these observations, without entering into theoretical considerations, will be best given by extracting certain passages from a memoir in Schumacher's Jahrbuch for 1836, entitled, "*Ueber Erdmagnetismus und Magnetometer*;" and adding the mathematical expressions of the laws there given verbally.

"The square of the number of vibrations made by a magnetic needle in a given time, is a measure of the intensity of the earth's magnetism which depends on the needle employed. The individual properties of the needle have a two-fold influence:—first, by the greater or less magnetic force which it possesses; and secondly, by the effect of its form and weight on the time of vibration. The elimination of the latter effect presents no difficulty. The influence of the earth's magnetism on the magnetism of the

needle produces a force or moment of rotation when the needle is not in the magnetic meridian: this moment of rotation is greater, the more the needle deviates from the magnetic meridian; and is greatest when the needle is at right angles to that meridian. This maximum of effect is always to be understood when the moment of rotation simply is spoken of; it may be represented by a given weight acting on a lever of given length, and consequently by a number, if the weights and lengths are expressed in numbers, according to arbitrary units. Now this moment of rotation and the time of vibration are very simply connected, by means of an intermediate quantity, dependent on the figure and weight of the needle, called its moment of inertia, and which may be calculated according to known rules. If the needle is not a perfectly regular body, or if it carries any appendage when in vibration, other means are required for the determination of its moment of inertia, the description of which would lead us too far; suffice it to say, that it is always possible. The moment of inertia then being known, the moment of rotation produced by the earth's magnetism on the magnetism of the needle, may be concluded from the observed time of the vibration of the needle."

If we designate by the letter *C*, the moment of inertia, after it has been multiplied by π^2, *i. e.*, 9·8696 . . . and divided by twice the height of the fall of a heavy body in the unit of time, we may conclude from *C*, and from the observed time of vibration *t*, the greatest moment of rotation caused by the earth; it is

$$= \frac{C}{t^2}.$$

"It is possible to determine the moment of rotation by direct experiment, without observing the time of vibration. An apparatus, expressly adapted to this purpose, has been recently placed in the Göttingen Astronomical Observatory, and is susceptible of great accuracy; but for the present purpose it is unnecessary to dwell on this point.

"The moment of rotation, produced by the earth's magnetism on a given needle, offers a new way of measuring the force of the earth's magnetism, or, to speak more accurately, a new form of the previous mode of measurement, over which it has this advantage, that one portion of the individuality of the

needle is thereby removed. The measurement is still dependent on the remaining peculiarity of the needle, namely, its own magnetism; and as soon as we can reduce *this* to an absolute measure, the force of terrestrial magnetism itself may also be reduced to an absolute measure; for we have only to divide the number which expresses the moment of rotation by the number which measures the magnetism of the needle. In fact, the earth's magnetism is measured by a force equal to itself, whose action on the unit of magnetism of the needle consists in a moment of rotation, measured by the force which the unit of weight exerts on a lever of the unit of length."

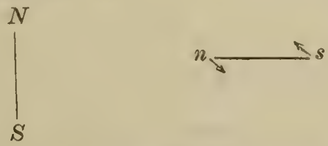
If, therefore, T signifies the terrestrial magnetism, and M the magnetism of the needle, or of the vibrating bar,

$$T = \frac{C}{t^2 . M} \qquad \text{(I.)}$$

"We might be inclined to suppose that the weight which a magnetic needle can carry would afford a standard by which the force of magnetism developed in the needle might be estimated; but a closer examination will show that this method is quite unavailing for our purpose. The determination itself is incapable of much precision; for repeated experiments give very different results; but there is a still more important objection: the capability of sustaining weight has no necessary connexion with the magnitude of the development of magnetism in the needle, in the sense in which it must here be understood. The moment of rotation is due to the magnetism of all the parts of the needle, upon which the terrestrial magnetism acts equally, and in parallel directions. The sustaining power, on the contrary, is chiefly due to the magnetism situated in the ends nearest to the weight, which, moreover, is modified every moment by the reciprocal action of the magnet-bar and the suspended iron.

"A magnetic needle, at a given place, acts on every point of space, in an amount and direction determined by its distance and position. In the immediate neighbourhood its action is strong, but very unequal on different parts; at great distances the action is weak, but almost uniform in strength and direction within a moderate space. The greater the distance, the nearer the law of the force approaches to a rule, which is very simple, and is completely given by theory: we may limit ourselves here to the consideration of a single case, which is sufficient for our purpose.

"Let $N S$ be a fixed magnet in a horizontal position; it is required to find its influence on a second needle, $n\ s$, suspended to a thread; the relative position of the two needles being shown in the annexed figure:



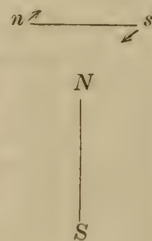
The action of the first needle upon the second will consist in imparting to it a tendency to turn in the direction indicated by the arrows, the letters N, n designating the North poles, and S, s the South poles. The moment of rotation is expressed by a number, exactly in the same way that the action of terrestrial magnetism on a needle vibrating freely has been indicated above. The magnitude of the moment of rotation depends, however, on the distance, and on the magnetic force in *both* needles. Thus for example (supposing the distances to be sufficiently great), at equal distances it would be augmented six-fold, by doubling the magnetism of one of the needles and trebling that of the other.

"The effect depends on the distance in such manner, that, at twice the distance the effect will be $\frac{1}{8}$th, and at three times the distance $\frac{1}{27}$th of the effect produced at the simple distance; bearing in mind, however, that this law is correct only for very great distances, and cannot be extended to small ones. As all distances, when referred to a selected unit, can be expressed by numbers, this law may be expressed thus: 'the moment of rotation, multiplied by the cube of the distance, is constant for very great distances.' This product may be termed with propriety the moment of rotation *reduced to the unit of distance*; remembering that, according to the remark above made, the actual moment of rotation at the unit of distance, when the distance is small, may differ considerably from the reduced moment. This, however, does not prevent us from employing the reduced moment of rotation as a measure of the magnetism of the needles, *and from considering as unity, the magnetism of that needle, which imparts to another needle, (of equal magnetism, and in a given position) a reduced moment of rotation equal to the effect of the unit of weight on the arm of a lever whose length is the unit of distance.*"

If we represent, according to this established unity, the magnetism of the needle by m, that of the bar by M, the distance (supposed considerable) between them by R, and the moment of rotation exerted by the bar on the needle by f, the reduced moment of rotation is

$$mM = fR^3$$

The position of the bar relatively to the needle, assumed in this case, did not in fact exist in the Göttingen experiments, but a different position represented in the annexed figure. However, the same thing is true of the two positions, with this single difference, that f has a different value, which we shall designate by F. In the Memoir "*Intensitas*," &c., it is proved that



$$F = 2f$$

so that,

$$mM = \frac{FR^3}{2} \qquad \text{(II.)}$$

It is to this second case that the formulæ, hereafter to be mentioned, will refer, as applicable to the Göttingen observations.

"In this way therefore we have a complete and precise idea of the measure of the magnetic force of a magnetized needle. A needle of twofold power will impart to one equally magnetized a reduced moment of rotation = 4; and generally, when we know the number for the reduced moment of rotation which a needle imparts to another needle equally magnetized, we have the absolute measure of the power of magnetism in each needle; it being the square root of that number.

"There only remains then, in order to be able to reduce the force of terrestrial magnetism to absolute measures, to give some method by which the moment of rotation which a needle produces in a similar one at considerable distance, (and in the position represented in the figure) may be determined with precision. A great difficulty might at first appear, from a circumstance purposely omitted in what has been already said, viz. the impossibility of observing this very weak action of the needle NS upon the needle ns, (which we will for a time suppose to be magnetized exactly as strongly as NS); since it cannot be withdrawn from the omnipresent, and much more powerful ac-

tion of the earth's magnetism. But this very circumstance affords the means of an easy solution. Let us suppose that in our figure the straight line, from the centre of the magnet $N\,S$, through the needle $n\,s$, coincides with the magnetic meridian; in this position the terrestrial magnetic force will not act at all on the needle $n\,s$. As soon, however, as the moment of rotation which $N\,S$ exerts on $n\,s$ begins to act, $n\,s$ will be deflected from its original position, and set in motion; but the more it deviates on account of this movement from its first position, the more strongly does the earth's magnetism tend to bring it back to its former position. The needle consequently performs vibrations about a line, which is no longer in the direction of the magnetic meridian itself, but is more or less inclined to it. This line is the position of equilibrium of the needle $n\,s$, which it assumes when the vibrations have ceased. This direction is evidently that of the resultant of the two forces, viz. the earth's magnetism, and the magnetism of the needle $N\,S$. According to the well-known laws of statics, the proportion of the strength of these forces, which is also the proportion of the moments of rotation produced by them, may consequently be determined from the angle of deviation, i. e. from the difference between the two positions of repose of $n\,s$, when it is subjected to the action of both the forces; and when $N\,S$ is removed.

"Here then arises another important remark; namely, that the angle of deviation of the needle $n\,s$ is quite independent of its magnetism; as any increase in that respect evidently causes both moments of rotation to increase in the same proportion. We are thus freed from the necessity of fulfilling the difficult condition of equality in the magnetism of the two needles."

If we represent the deflection by v,—the greatest moment of rotation exerted by the earth on the needle (according to the measure fixed for the terrestrial magnetism) by $m\,T$,—and by F, the moment of rotation exerted by the magnetism of the bar (M) on the magnetism of the needle (m) at the distance R; the forces exerted by the earth, and by the bar, on the needle, are to each other in the proportion of the cosine to the sine of the deflection v; and the moments of rotation, $m\,T$ and F being also in the same relation to each other,

$$m\,T : F :: \cos v : \sin v,$$

i. e.

$$m\,T = \frac{F}{\operatorname{tang} v}. \qquad \text{(III.)}$$

If we divide the equation (II.) by (III.) we obtain

$$\frac{m\,M}{m\,T} = \frac{F\,R^3 \,.\, \text{tang}\, v}{2\,F},$$

whence the independence of the deflection v, both of the magnetism of the needle m, and of the moment of rotation F, is evident of itself, and we have the following simple result:

$$\frac{M}{T} = \frac{R^3\, \text{tang}\, v}{2} \qquad \text{(IV.)}$$

" The determination of the intensity of the magnetism of the globe is therefore reduced to two principal operations.

" I. To observe the time of vibration of a needle $N\,S$, and to deduce from thence the moment of rotation which the terrestrial magnetism exerts on it."

This moment of rotation will be expressed by the product $M\,T$, and calculated by the equation (I.)

$$T = \frac{C}{M\,t^2}, \quad \text{or } M\,T = \frac{C}{t^2}$$

in which C represents the moment of inertia of the bar, multiplied by the number, π^2, or 9·8696 . . . and divided by the double of the space of the fall of a heavy body in the unit of time.

" II. A second needle, $n\,s$, being suspended: its position is observed; first, when subject to the influence of the earth's magnetism alone; and secondly, after $N\,S$ has been placed at a considerable distance, as represented in the figure. Then calculate from the difference between the two positions, or from the deflection, what fraction of the force of the earth's magnetism, the magnetic force of the needle $N\,S$, corresponds to at the selected distance. An equal fraction of the moment of rotation, found in (I.,) gives the moment of rotation which the needle $N\,S$ at that distance would impart to a similar one; this result, multiplied by the cube of the distance, gives the reduced moment of rotation; the square root of this gives the force of the needle $N\,S$ in absolute measure: and finally, the number found in (I.) divided by this square root, gives the expression for the absolute measure of the earth's magnetism."

The ratio which the force of the bar on the needle (at the given distance R) bears to that of the earth's magnetism is expressed by the quotient

$$\frac{F}{m\,T}$$

and according to equation (III.)

$$m\,T = \frac{F}{\text{tang}\;v}, \qquad \text{or} \qquad \frac{F}{m\,T} = \text{tang}\;v.$$

But according to equation (II.)

$$m\,M = \frac{F R^3}{2}, \qquad \text{or} \qquad \frac{F}{m\,T} = \frac{2\,M}{R^3\,T},$$

and,

$$\frac{2\,M}{R^3\,T} \,.\, M\,T = \frac{C}{t^2} \,.\, \text{tang}\;v,$$

makes known to us the maximum moment of rotation which the bar with the magnetism M would exert on a similar bar at the distance R; for this maximum, according to the fundamental laws of magnetism, must be $\frac{2\,M^2}{R^3}$; and the above equation, gives $\frac{2\,M^2}{R^3} = \frac{C}{t^2}.\,\text{tang}\;v.$

This result, multiplied by the cube of the distance R, gives double the moment of rotation; and the square root of the half gives the force of the bar in absolute measure; or

$$M = \frac{1}{t}\sqrt{\frac{C\,R^3\,.\,\text{tang}\;v}{2}}. \qquad \text{(V.)}$$

If, finally, we divide by the moment of rotation exerted by the earth on the needle, calculated according to equation (I.) we obtain

$$T = \frac{1}{t}\sqrt{\frac{2\,C}{R^3\,\text{tang}\;v}}, \qquad \text{(VI.)}$$

and this number expresses the absolute measure of the earth's magnetism.

"This appears the most easily understood exposition which can be given, without the use of mathematical signs, of the possibility of expressing the force of the earth's magnetism by a number which shall be perfectly independent of the individuality of the magnetic bars employed. In the actual application some points will appear in a somewhat different form, without, however, affecting in the least the nature of the method; and it will, besides, be necessary to take into consideration several collateral circumstances."

We will add a few remarks on one or two circumstances.

"In speaking of the units to be employed in the measurements, mention was made only of a unit of distance and a unit of weight. But it should not be overlooked, that a certain weight, (a gramme, for instance,) does not mean, in this case, the quantity of ponderable matter which bears this name, and which is everywhere the same,—but the force which this quantity of matter exerts at the place of observation, under the influence of gravitation. It is well known that the force of gravity is not absolutely the same at different places; and if we chose the force of a gramme for our unit of weight, the intensity of the earth's magnetism would not be accurately measured by one standard at various places. The accuracy with which these measurements may now be made is such that this difference must not be neglected. The most simple way of meeting this difficulty is to reduce the force of gravity itself to an absolute quantity, by adopting as its measure the double height of descent in the unit of time, (for instance, a second,) and by expressing the force by the product of the mass into the number which measures the force of gravity. In this manner other numbers* are obtained, both for the force of the magnetic needle employed, and for that of the earth's magnetism; which numbers are based on three units, *i. e.*, a unit of distance, a unit of time, and a unit of mass—instead of resting on the two units before spoken of."

In calculating the numbers M and T according to equations (V.) and (VI.)

$$M = \frac{1}{t}\sqrt{\frac{C R^3 . \operatorname{tang} v}{2}} \qquad T = \frac{1}{t}\sqrt{\frac{2\,C}{R^3 \operatorname{tang} v}},$$

the value ascribed to the constant C was

$$C = \frac{\pi^2}{g} . K,$$

in which π represented the known number 3·14159 . . . ; g, double the space of descent in the unit of time; K, the moment of inertia of the vibrating bar. The new numbers are obtained from the same equations, by ascribing to C the value

$$C = \pi^2 K.$$

* They are to the previous numbers in the proportion of the square root of the number which measures the force of gravity to unity.

"One main difficulty in the application of this method consists in the fact, that the above-mentioned law holds good, namely, that the action of a magnetic needle is inversely as the cube of the distance, with sufficient accuracy only for very great distances, and where the effects are too small to be determined with precision by direct observation. At moderate distances the variations from the law become very perceptible; but theory teaches that these very differences are subject to rule; and mathematics afford us the means of recognising, and almost wholly eliminating them, by the combination of experiments made at various moderate distances."

For the purpose of showing the application of the small measuring apparatus to the above-mentioned observations, we shall give lastly, in a few words, the necessary process of correction. This is threefold:

1. Instead of the values given by direct observation for the deflexions v_0, v_1, v_2, etc., of the needle by the magnet bar acting at various distances, R_0, R_1, R_2, etc., the following combined values are to be taken:

$$v_0 = \tfrac{1}{4}\,(u_0 - u_0{}' + u_0{}'' - u_0{}''')$$
$$v_1 = \tfrac{1}{4}\,(u_1 - u_1{}' + u_1{}'' - u_1{}''')$$
$$v_2 = \tfrac{1}{4}\,(u_2 - u_2{}' + u_2{}'' - u_2{}''') \text{ etc.}$$

2. To the approximate values of $\frac{M}{T}$, which were obtained by equation (IV.)

$$\frac{M}{T} = \frac{R^3 \operatorname{tang} v}{2}$$

the following corrections are added:

Approximate value of $\frac{M}{T}$.	*Correction.*
$\frac{R_0{}^3 \operatorname{tang} v_0}{2}$	$-\frac{L}{R_0{}^2}$
$\frac{R_1{}^3 \operatorname{tang} v_1}{2}$	$-\frac{L}{R_1{}^2}$
$\frac{R_2{}^3 \operatorname{tang} v_2}{2}$ etc.	$-\frac{L}{R_2{}^2}$ etc.

3. As the number of the measured dimensions R_0, R_1, R_2, etc. and v_0, v_1, v_2, etc., is greater than is required for the determination of the unknown quantities L and $\frac{M}{T}$, the rules of the calculus of probabilities are employed in order to obtain from them the

most probable values of those quantities. These rules are as follows.

From the quantities R_0, R_1, R_2, etc., v_0, v_1, v_2, etc., we must calculate the following expressions:

$$\frac{\text{tang } v_0}{R_0^3} + \frac{\text{tang } v_1}{R_1^3} + \frac{\text{tang } v_2}{R_2^3} + \text{etc.} = A$$

$$\frac{\text{tang } v_0}{R_0^5} + \frac{\text{tang } v_1}{R_1^5} + \frac{\text{tang } v_2}{R_2^5} + \text{etc.} = A'$$

$$\frac{1}{R_0^6} + \frac{1}{R_1^6} + \frac{1}{R_2^6} + \text{etc.} = B$$

$$\frac{1}{R_0^8} + \frac{1}{R_1^8} + \frac{1}{R_2^8} + \text{etc.} = B'$$

$$\frac{1}{R_0^{10}} + \frac{1}{R_1^{10}} + \frac{1}{R_2^{10}} + \text{etc.} = B'';$$

thence we shall have the most probable value of

$$L = \tfrac{1}{2} \cdot \frac{AB' - A'B}{B'^2 - B''^2}$$

$$\frac{M}{T} = \tfrac{1}{2} \cdot \frac{A'B' - AB''}{B'^2 - B''^2} = r.$$

From this, and equation (I.)

$$MT = \frac{C}{t^2}$$

we obtain

$$M = \frac{1}{t}\sqrt{rC} \qquad \text{(VII.)}$$

$$T = \frac{1}{t}\sqrt{\frac{C}{r}}. \qquad \text{(VIII.)}$$

The experiments with the small measuring apparatus may be calculated according to these laws and formulæ, and the absolute magnetism of the bar and that of the earth determined.

4. *Calculation, according to the above rules, of the observations made with the small measuring apparatus.*

The experiments were, 1st, those of deflection, which gave the values of $u_0 - u_0'$, $u_1 - u_1'$, $u_2 - u_2'$, $u_2'' - u_2'''$, $u_1'' - u_1'''$, $u_0'' - u_0'''$, and the corresponding values of R, viz. R_0, R_1, R_2, R_2, R_1, R_0. We calculate from these the values of v_0, v_1, v_2, corresponding to R_0, R_1, R_2; and hence the values of A, A', B, B', B'', which are simple functions of the six quantities, v_0, v_1, v_2; R_0, R_1, R_2. And lastly, the value of r is deduced from the quantities A, A', B, B', B'', of which it is a function. Thus, the value

of r is obtained by calculation from the experiments of deflection.

2nd. From the experiments of vibration the value of the time of vibration t is found: having thus the values of r and t, it is only required, for the purposes of the travelling observer, to calculate

$$\frac{1}{t\sqrt{r}};$$

for this value is proportional to the number, which expresses the absolute terrestrial magnetism, and consequently suffices for the *comparison* of the absolute intensity of all places where such experiments may be performed. Such a *comparison* is usually the only object sought by the travelling observer. It may sometimes, however, be desirable to obtain not only comparisons of the absolute intensity at various places, but the absolute intensity itself; the apparatus may be lost on a voyage, and be replaced by a new one; and it then becomes necessary, in order to compare the two series of results obtained with instruments which cannot be compared together, to calculate the moment of inertia of the magnet bar, the time of vibration of which had been observed, and to extract its square root. The product of the quantity $\frac{1}{t\sqrt{r}}$ into the square root, and into the number $\pi = 3{\cdot}14159\ldots$ gives a number expressing the earth's magnetism in absolute measure.

On this account it is advantageous that the bar should be an accurate parallelopiped, because in such case the moment of inertia can be deduced for the present purpose directly from the weight p, the length a, and the breadth b of the bar. For it is well known that the square $a^2 + b^2$ of the diagonal of the superficies of the parallelopiped, multiplied by the mass p of the weight, and divided by 12, gives the moment of inertia sought, in the case in which the bar shall have been suspended by the centre of that superficies. Consequently in the equations (VII.) and (VIII.)

$$C = 9{\cdot}8696 \cdot\cdot \frac{a^2 + b^2}{12} . p$$

If we compare the observations above mentioned with these formulæ, it will be seen that the following quantities have been directly measured, and the following values found for them:

$$u_0 - u_0' = 23^\circ \ 9'$$
$$u_1 - u_1' = 47^\circ \ 42'$$
$$u_2 - u_2' = 71^\circ \ 48'$$
$$u_2'' - u_2''' = 69^\circ \ 21'$$
$$u_1'' - u_1''' = 46^\circ \ 12'$$
$$u_0'' - u_0''' = 22^\circ \ 27'$$

$$\left.\begin{array}{l} R_0 = 450 \\ R_1 = 350 \\ R_2 = 300 \end{array}\right\} \text{millimetres}$$

$$t = 6''\cdot 67$$

$$\left.\begin{array}{l} a = 101\cdot 0 \\ b = \ \ 17\cdot 5 \end{array}\right\} \text{millimetres.}$$
$$p = 142000 \text{ milligrammes.}$$

From these may next be calculated,

$$v_0 = \tfrac{1}{4}(23^\circ \ 9' + 22^\circ \ 27') = 11^\circ \ 24'\cdot 00$$
$$v_1 = \tfrac{1}{4}(47^\circ \ 42' + 46^\circ \ 12') = 23^\circ \ 28'\cdot 50$$
$$v_2 = \tfrac{1}{4}(71^\circ \ 48' + 69^\circ \ 21') = 35^\circ \ 17'\cdot 25$$

If now we take the second and the millimetres as the fundamental units of time and space in our calculation, we may deduce from the ascertained values of R_0, R_1, R_2, v_0, v_1, v_2, the following values of A, A', B, B', B'', viz.

$$A = \frac{\text{tang } 11^\circ \ 24'}{450^3} + \frac{\text{tang } 23^\circ \ 28'\cdot 5}{350^3} + \frac{\text{tang } 35^\circ \ 17'\cdot 25}{300^3} = \frac{385\cdot 54}{10^{10}};$$

$$A' = \frac{\text{tang } 11^\circ \ 24'}{450^5} + \frac{\text{tang } 23^\circ \ 28'\cdot 5}{350^5} + \frac{\text{tang } 35^\circ \ 17'\cdot 25}{300^5} = \frac{384\cdot 86}{10^{15}};$$

$$B = \frac{1}{450^6} + \frac{1}{350^6} + \frac{1}{300^6} = \frac{2\cdot 0362}{10^{15}};$$

$$B' = \frac{1}{450^8} + \frac{1}{350^8} + \frac{1}{300^8} = \frac{2\cdot 0277}{10^{20}};$$

$$B'' = \frac{1}{450^{10}} + \frac{1}{350^{10}} + \frac{1}{300^{10}} = \frac{2\cdot 0855}{10^{25}}.$$

From these r is calculated:

$$r = \tfrac{1}{2} \cdot \frac{385\cdot 54 + 2\cdot 0855 - 384\cdot 86 + 2\cdot 0277}{2\cdot 0362 + 2\cdot 0855 - (2\cdot 0277)^2} \cdot 10^5$$

or

$$r = 87650000.$$

Finally, from this value of r, and from that of t, determined by observation, may be deduced the value:

$$\frac{1}{t\sqrt{r}} = \frac{1}{6{\cdot}67 \cdot \sqrt{8765000}} = \frac{5{\cdot}0641}{10^5}.$$

This number suffices for the comparison of all intensities measured with the same instrument, however the magnetic condition of the apparatus may have varied.

Further, the number T, which expresses in absolute measure the resulting intensity of the earth's magnetism, may be ascertained by deducing from the observations the value of C, and multiplying the former number by its square root. C is calculated from the observed values of a, b, and p, the mass of the milligramme being taken as the unity of mass:

$$C = 9{\cdot}8696 + \frac{101^2 + 17{\cdot}5^2}{12} + 142000 = 0{\cdot}12272 + 10^{10}$$

whence T is deduced

$$T = 5{\cdot}0641 \,.\, \sqrt{0{\cdot}12272} = 1{\cdot}774.$$

5. *Examination of the result.*

This number 1·774, expressing the intensity of terrestrial magnetism on the 18th of January, 1837, possesses, as an absolute measure, the advantage of being directly comparable with the results obtained in 1834 with the magnetometer of the Göttingen magnetic observatory, published in the *Göttingen gelehrten Anzeigen* of that year. They will be found in part 128, (with the account of the newly-constructed building, and of the instruments, as well as of the first experiments performed there). They are as follow:

July 17	1·7743
— 20	1·7740
— 21	1·7761

Two apparatus destined for the same purpose can hardly be more dissimilar than the small apparatus above described, and the magnetometer. It results from the comparison, that the intensity of the terrestrial magnetism in Göttingen has undergone hardly any alteration from 1834 to 1837.

We have also a direct comparison of this number obtained for Göttingen with the result of observations with a third apparatus, differing widely from both the others made at Munich, April 1st, 1836, viz. 1·905, and with the number found for

Milan, with the magnetometer of that place, in October, 1836, viz. 2·01839.

To gain a clear idea of the import of these numbers, the determination and application of which have been hitherto under consideration, imagine a number of small steel bars, perfectly alike, and each weighing about $2\frac{1}{2}$ grammes, or $\frac{1}{6}$ of an ounce. Imagine further a balance, of which the length of the arms bears to 1 metre the same proportion that 1 metre bears to the space of descent in 1 second (204 millimetres nearly); suppose one of these steel bars to be attached in a parallel direction to the horizontal beam of the balance, in such manner that the equilibrium is not thereby disturbed. Then render all the steel bars (including the one attached to the balance) equally magnetic, and to such a degree that when another of their number is placed vertically beneath the scale at the distance of 1 metre from the attached magnet bar, $\frac{1}{1000}$th of a milligramme must be placed in the scale to preserve equilibrium. When the magnetism of all the bars has been regulated in this manner, place one of the bars horizontally, and at right angles to a small compass needle, 1 metre from the centre of the needle beneath, taking care that as the compass needle is deflected from the magnetic meridian, the bar be also turned so that they may preserve their rectangular position. Lastly, calculate how many such bars are required that their united force may deflect the compass needle 90°; the number of bars gives the terrestrial magnetism in thousandths of its absolute measure.

We may conceive in like manner the number which represents the absolute measure of the terrestrial magnetism to represent the number of these bars reckoned in thousands, the forces of which must be united to cause, at a distance of a metre, a deviation of 90°. This would require at

Göttingen the force of . . .	1775 bars
Munich	1905 —
Milan	2018 —

6. *On the Advantages of the Dimensions selected for the small Measuring Apparatus.*

Before concluding this article, we have to discuss the accuracy of which the absolute measurement of intensity with the apparatus described is susceptible, and on what it is founded. It has been already remarked, that the absolute intensity can be

measured with the accuracy it deserves only with the magnetometer. It is therefore unnecessary to state that such extreme accuracy cannot be attained with the small apparatus. And in order to obtain with it a good approximation, it must combine all the advantages of which it is susceptible.

The difficulty of an accurate measurement of intensity, with other instruments than the magnetometer, is thus stated in the memoir "*On Terrestrial Magnetism and the Magnetometer*:"

"In all cases, if the elimination is to be satisfactory, the experiments must not be performed at too small distances; consequently the effects are always comparatively small, and the means previously in use are inadequate to measure them with the necessary precision. It is this difficulty which has called for, and has given rise to the construction of a new apparatus, which may with propriety receive the name of *magnetometer*, since it serves to execute, with an accuracy equaling that of the most delicate astronomical determinations, all measurements—both of the force of magnetic needles, and of the intensity of the earth's magnetism (at least its horizontal portion). The (horizontal) direction of the earth's magnetic force is determined accurately with it to within one or two seconds of arc; the commencement and termination of a vibration is observed with it to within a few hundredths of a second of time, and consequently more accurately than the passage of stars behind the wires of a transit."

There are two circumstances, chiefly, on which the accuracy of an absolute measurement of intensity depends; *first*, the magnitude of the deflection produced; *secondly*, the delicacy of the instrument in measuring this deflection. In constructing an apparatus for this purpose we may therefore follow two different paths: we may either make the amount of deflection the main object, and pay only as much attention to the means of measurement as may be consistent therewith;—or we may attend chiefly to accuracy in the means of measurement, and let the amount of the deflection be the second object. The latter plan leads to much greater accuracy than the former, for this reason: the amount of deflection soon attains a limit, on account of the necessary condition of a considerable distance between the deflecting bar and the needle, so that the deflection produced

must always be small. If, however, all pretensions to great accuracy of measurement are relinquished at the outset, by making the magnetic needle play on a pivot, instead of suspending it by a silk thread, the friction of the point renders fineness of measurement quite illusory, and the former much less advantageous plan is the only one that remains open; the endeavour must then be to adopt the arrangements and proportions best suited to produce the greatest possible deflection. This is the express object of the small size of the apparatus described, and not merely to render it light and convenient of transport.

That the small size of the apparatus does actually allow of a great amount of deflection is evident by the result; for in the experiments above mentioned all the measured angles exceeded 20°: it is easy to explain the reason.

1. The distance of the deflecting bar from the needle must be *relatively* great, but need not be *absolutely* so: it must at least be three or four times greater than the length of the deflecting bar, or of the magnetic needle.

2. By diminishing in proportion all the linear dimensions of the apparatus (viz. the dimensions of the magnets, and their distance apart), the angular magnitudes, of which the deflection is one, remain unchanged; therefore such proportional reduction in the size of the apparatus causes no loss in the amount of the deflection to be measured.

3. But if instead of diminishing in equal proportion all the linear dimensions of the apparatus, we diminish only the length of the magnets and their distance apart, the breadth and thickness of the deflecting bar being little or not at all diminished, then we even gain an increase in the angular magnitudes, and it only remains to know how far this increase may be carried.

The limit depends on a single circumstance, viz. on the breadth and thickness of the deflecting bar, with a given length. Experience has shown, that neither the breadth nor the thickness of the bar ought to exceed the eighth part of its length. It follows that the greatest deflection may be produced by a magnet bar, of which the breadth and the thickness are equal, and of which the length is eight times greater than either, and acting upon a magnetic needle, placed at a distance equal to three or four times the length of the bar; the length of the needle must not exceed that of the bar.

From this rule then we obtain the most advantageous dimensions of such an apparatus, by knowing the limit in respect to thickness, which is determined by the *nature of the steel.* The thickness of the bar must not amount to much more than $12\frac{1}{2}$ millimetres, as otherwise the steel cannot be properly hardened and magnetized throughout. We thence obtain the following dimensions of the deflecting bar, as those which combine the greatest advantages, namely, for its breadth and thickness $12\frac{1}{2}$ millimetres, and for its length 100 millimetres. We have also the length of the magnetic needle 100 millimetres, and the smallest admissible distance between them, 300 millimetres.

By following these rules we obtain an apparatus, with which, in mean latitudes, the smallest deflections to be measured exceed 22°, as in the experiments related. At greater distances from the magnetic poles of the earth, this deflection becomes somewhat smaller; nearer to the magnetic poles it is much larger. Therefore, if these deflections can be accurately measured to within a tenth part of a degree, a final result can be obtained to within the 200th part of the force itself; since all other measurements required in the determination of the absolute intensity can be made with greater accuracy. This result, it is true, is far inferior to that which can be obtained with the magnetometer; but such results may still be of great utility in the absence of more accurate determinations.

WEBER.

V.

Explanations of the graphical representations, and of the table of results.

In Plates IV.–IX. are given the graphical representations of the changes of declination during six terms, amounting, in all, to forty-six curves, from fourteen stations, viz. Berlin, Breda, Breslau, Catania, Freiberg, Göttingen, the Hague, Leipzig, Milan, Marburg, Messina, Munich, Palermo, and Upsala. The graphical representations begin with the November term of 1835, when the Association was strengthened by the accession of several new and zealous cooperators. The representations of two terms of the year 1836 have been omitted, viz. those of March and May, as the changes they present are comparatively less interesting than those of the two terms of January and July, be-

tween which they occur; and the number of six terms, fixed on as the rule for the annual publication, is completed by the addition of an extra term in August.

Of the apparatus employed, three are exactly similar to those at Göttingen, but of smaller dimensions; namely, that of Dr. Wenkebach, first used at the Hague, and subsequently at Breda; the travelling apparatus with which M. Sartorius of Waltershausen, and Dr. Listing, observed in Palermo, Catania, and Messina; and the apparatus already mentioned at p. 22, in the Berlin Magnetic Observatory, which latter, however, will be shortly replaced by a larger one, of Meyerstein's. The other apparatus in Breslau, Freiberg, Göttingen, Leipzig, Milan, Marburg, Munich, and Upsala, are all alike.

The participators in the observations represented in the six terms, as far as the names have come to our knowledge, were as follows:

In *Berlin*, besides Prof. Encke, MM. Bremiker, Galle, Mädler, and Wolfers.

In *Breslau*, besides Prof. V. Boguslawski and his sons, MM. Bratke, Brier, Dittrich, Höniger, Jacobi, Isaac, Klingenberg, Koch, Körber, Küntzel, Maywald, Müller, Dr. Pappenheim, Reichelt, Reisern, Ribbeck, Riemann, Roedsch, Wiedemann, and Wilde.

In *Catania*, Dr. Listing, MM. Sartorius von Waltershausen, and Zobel.

In *Freiberg*, besides Prof. Reich, MM. Felgner, Neubert, and Walther.

In *Göttingen*, MM. Bräss, Lieut. Engelhard, Dr. Goldschmidt, Meyerstein, Schröter, Dr. Stern, Lieut. von Stolzenberg, Prof. Ulrich, Dr. Wappäus, Dr. E. Weber, and Prof. W. Weber.

At the Hague, (in the September term,) besides Dr. Wenkebach, MM. von Cranenburgh, Rueb, and Simons.

In *Leipzig*, besides Prof. Möbius, MM. Brandes, Faber, Hülse, Kühne, Michaelis, Netsch, and Zunck.

In *Milan*, besides M. Kreil, MM. Capelli, Stambucchi, and Della Vedova.

In *Marburg*, besides Prof. Gerling, MM. Beck, Deahna, Eichler, Fliedner, Hartert, Hartmann, Ise, Kutsch, Landgrebe, Lotz, and Oppermann.

In *Messina*, Dr. Listing, MM. Sartorius von Waltershausen, and Tardy.

In *Munich*, besides Prof. Steinheil, MM. Hierl, Lamont, Lippolt, Meggenhofen, Mielach, Pauli, Pohrt, Recht, Schleicher, Schröder, Siber, and Zuccarini.

Other observations of some of these six terms have also come to our hands, but too late for insertion in the plates; this is the more to be regretted, as, for the most part, they accord with the others in a very interesting manner. The results of the observations made at Upsala, in the September term, 1836, which are of this kind, are printed in the sequel. The Milan observations of November, 1835, which were also received after the curves for the six other stations had been drawn on stone, were inserted below them; but for this circumstance, their place would have been between the Munich and Palermo observations. The Göttingen observations have required no process of reduction, being drawn in accordance with the divisions of the scale as indicated in the margin, the height of each square being taken as two divisions of the scale in all the terms, with the single exception of that of January, 1836. The changes during that term are the greatest which have been hitherto observed, and rendered it necessary, in order not to increase the height of the page too much, to allow three divisions of the scale for each square. Increasing numbers always denote an advance of the needle from right to left,—in other words, diminishing westerly variations. The observations at Breslau, Freiberg, the Hague, and Leipzig, where the divisions of the scale are nearly of the same magnitude as in Göttingen, have been drawn according to the same proportion. The distance between the curves is an arbitrary quantity in each case, determined solely by its fulfilling the one object of keeping them at a convenient distance apart.

For those stations where the value of the divisions of the scale differs considerably from that at Göttingen, the original numerical results were multiplied in each case by a common factor, expressing, as nearly as possible, in convenient numbers, the proportion to the Göttingen scale. Thus, the various curves in each term are represented very nearly according to a common scale. In the January term alone the scale of representation is somewhat more unequal, the cause of which does not merit any mention in this place, as it suffices to know the scale for each curve. In the three first terms the height of each square corresponds to the following values of arc, viz.:

	November, 1835.	January, 1836.	July, 1836.
Hague	42''·01	63''·01	42''·01
Göttingen	42·25	63·38	42·25
Berlin	—	—	42·24
Breslau	—	—	42·40
Leipzig	41·34	63·01	41·34
Marburg	42·20	60·28	42·20
Munich	41·86	55·82	41·86
Milan	40·27	60·40	41·33
Palermo	42·07	—	—
Catania	—	41·56	—
Messina	—	—	43·06

For the three last terms, the value of the divisions of the scale, and the proportion, according to which they have been inserted in the plates, are stated in the table of numerical results.

The curves are all drawn according to Göttingen mean time, (indicated at the top of each plate,) or at least very nearly so, and therefore contemporaneous movements appear all in one vertical line. The order in which the several curves are arranged in each plate was principally regulated by convenience as to the curves fitting into each other.

The following remarks may be added in regard to particular terms:

On the 28th of November, 1835, and during the following night, the observations at Palermo were much disturbed by an exceedingly violent Sirocco-wind, so that at one time they had even to be suspended for an hour and a half; and at other times only partial and uncertain determinations could be obtained. It is probable, therefore, that many of the apparent movements were not real magnetic changes. Nevertheless, we determined not to exclude this curve; as the latter part of it, from the morning of the 29th November, when the storm had nearly passed over, offers a sufficiently satisfactory accordance with the stations to the north. I take this opportunity of mentioning that, according to all our experience hitherto, the most violent storms of wind appear to be wholly without influence on the magnetometer, provided only the instrument is effectually protected from any effect of their direct mechanical action. Very frequently, either an extremely quiescent state of the needle, or a very regular and uniform progress, has been remarked in the Magnetic

Observatory of Göttingen, during the prevalence of the most violent storm. If any one, however, were inclined to infer from such experience, that storms in the atmosphere, on the other hand, counteract or enfeeble the magnetic forces, such an idea would be dispelled by what took place during the term of January 1836. During this term a very violent storm prevailed at Göttingen, and at many other stations; and several observers in other places accompanied the results which they communicated, by the expression of a fear that from this circumstance the unusually large movements shown by the magnetometer might offer but little accordance. Nevertheless, the harmony of the curves from the various stations was so complete (see the representations in Plate V.) that it might have been termed wonderful, if the same thing had not been manifested before by so many experiments. As with *wind* storms, so it is with *thunder* storms, which, even when close at hand, exercise (as attested by several cases which have occurred here and at other places) no perceptible influence on the magnetic needle*.

A letter from M. von Humboldt, received in August, 1836, contained the information that, from the 10th to the 18th of August, the magnetic changes would be observed uninterruptedly every quarter of an hour at Reikiavik, in Iceland, by a practised French astronomer, M. Lottin, with Gambey's apparatus, and expressed the wish that corresponding observations might be made on one or on some of those days with magnetometers. In consequence an unusual term was fixed for the 17th and 18th of August, and as far as the shortness of the time allowed, several members of our Association at other stations were invited to take part in it. This unusual term was observed in Upsala, the Hague, Göttingen, Berlin, Leipzig, and Munich, in exactly the same way as the usual terms; and if the graphically represented observations in Plate VII. exhibit exceedingly interesting changes, we have only to regret that the place reserved at the top of the plate for the Iceland observations is vacant, as we have not been able to obtain the slightest information respecting the result of the French Icelandic observations.

The September term presents a case which may be noticed somewhat in detail, as it confirms, in a very instructive man-

* There is, of course, no question here of experiments in which the atmospheric electricity is conducted to the earth by means of a conducting wire passing through a multiplier surrounding the needle.

ner, what has been stated at p. 50. In the register of the Marburg observations, which on that occasion were made in the absence of Prof. Gerling, and at the hours of $12^h\,0^m$, $12^h\,5^m$, and $12^h\,10^m$, there appeared an unusual irregularity, which excited the suspicion, that about $12^h\,5^m$ a spider had prevented the free motion of the needle by attaching a thread; and this suspicion was increased by the circumstance, that from $12^h\,10^m$ to the end, the changes of the needle were exactly similar to those which resulted from observations at other stations, but appeared proportionally much smaller than could have been expected from the experience of other terms. Prof. Gerling was requested, on his return to Marburg, to examine the apparatus carefully, and the result is contained in a letter from the Professor.

The examination took place on the 5th of November, up to which time no one had entered the room of observation since the September term. In the first place the position of the needle was determined and found as follows:

at $3^h\,33^m$	. . .	445·63
35	. . .	445·73
37	. . .	445·71

Upon this the needle was set in moderate vibration by means of the moderating bar, and hence a time of vibration of 17 seconds was found, being nine seconds less than the usual duration: the lid of the case was then carefully removed, and a very minute living spider was noticed on its under surface; a very small, and nearly imperceptible, thread was thought to be observed hanging to it: further, a number of small, black point-like bodies were found in the box, which, under the microscope, proved to be the dead bodies of gnats; and finally, in one corner of the box, a regular undisturbed web, of such fine texture, that without the reflexion of the light it would hardly have been perceptible. From all these circumstances it may be supposed that the spider had been some time in the box.

When the finger had been passed round the magnet bar in all directions, new observations of the time of vibration gave again the former value of 26 seconds. The position was also found to correspond to much lower numbers on the scale, namely,

$4^h\,43^m$	. . .	431·45
45	. . .	431·46
47	. . .	431·12

Of course, however, these observations could not furnish an accurate determination of the amount of error introduced, as the declination may have altered during the interval, which amounted to more than an hour.

In the graphic representation the second half of the Marburg curve has been drawn on a reduced scale, the reduced divisions representing 28 on the Marburg scale.

I may here mention a second case of a similar kind. The time of vibration of the magnet bar in Breslau, which, in March 1836, amounted to nearly 3·2 seconds, had from that period to November gradually increased, making altogether an increase of about 0″·4. This is no unusual circumstance, as all magnetic bars in the course of time lose some part of their force, though in very various degrees dependent on the unequal tempering of the steel and other circumstances. But, from November 1836, to January 1837*, a *decrease* in the time of vibration of 1″·27 took place. Prof. von Boguslawski, who informed me of this remarkable circumstance, seemed inclined to attribute it in part to an increased intensity of the terrestrial magnetism. I did not doubt, however, that the cause must be sought in the immediate neighbourhood of the magnet bar, probably in some impediment to its free motion, and this supposition was verified by the following letter of M. Boguslawski :—

"You were right in your supposition as to the cause of the alteration in the time of vibration. By a slight accidental displacement of the box, the edge of the small aperture through which the suspension thread passes, had been brought near the thread, though by no means into contact with it. However, some of the finer fibres of the silk must have been touched thereby, for when it was again made to pass quite through the centre of the aperture, the time of vibration was found almost identical with that formerly observed."

This is perhaps the place for some remarks on the movements themselves, which are here represented during six terms.

In the three summer terms, (Plates VI. VII. and VIII.) notwithstanding all the great anomalies, the regular diurnal movement is clearly seen in the curves, ascending during the hours

* Probably during the interval no determinations of the time of vibration had been made.

after noon, and descending in the following hours of the forenoon; there is, on the other hand, scarcely a trace of this in the three winter terms, Plates IV. V. and IX. All our experience shows that partial or even total obliteration of the regular movements by the irregular is a very common occurrence. In the years 1834 and 1835 some terms occurred in which the regular course was not at all obscured by any considerable anomalies, although there was no want of smaller ones. But what renders the anomalous oscillations so remarkable, is their extraordinary coincidence, generally even in the smaller instances, at different stations; nay, commonly at all the stations, only in dissimilar proportions of magnitude. It is quite unnecessary to demonstrate this agreement in individual instances: a view of the representations of the six terms will speak sufficiently for itself.

We cannot at present decipher these enigmatical hieroglyphics of nature: we must first endeavour to procure from the most diversified sources, authentic, numerous, and minutely faithful copies, in the confident hope, that when these rich materials are accumulated, the key to their hidden meaning will not be long wanting. In the mean time I may be allowed to add a few remarks, which may assist in the formation of a more correct judgement concerning them.

First, it must not be forgotten that these anomalies are but comparatively small modifications of some of the effects of the great terrestrial magnetic force; that we must distinguish between the force itself and these supervening alterations; and that nothing in the present state of our knowledge obliges us to ascribe both to the same or to similar causes. Therefore those who think it probable that these anomalies are the effects of electric currents, or of action, perhaps far beyond our atmosphere, (which view we leave entirely to its own merits) may continue to do so, without having to relinquish on that account the old view, of a force, residing in the solid portions of the earth, or rather being the collective action of all its magnetized particles. If, according to the opinions of some philosophers, the interior of the earth be supposed still in a fluid state, the constantly advancing solidification, and the consequent thickening of the solid crust of the earth, would offer the most natural explanation of the secular variations of the magnetic force.

But we willingly leave the uncertain ground of hypothesis,

and return to facts. By far the greater number of the anomalies are found to be smaller at the southern stations, and larger at the northern. For instance, the remarkable ascent of the curve, on the 30th January, 1836, between 9^h 25^m, and 9^h 40^m, amounted in Catania to 6′ (reduced to parts of arc); in Milan to 12′; in Munich to $13\frac{1}{2}'$; in Leipzig to 16′; in Marburg to 20′; in Göttingen to 26′; and at the Hague to 29′. Something, it is true, must be deducted from this inequality, due to the circumstance that, at the northern points (where the horizontal portion of the terrestrial magnetic force has a weaker intensity than at the more southern ones,) similar disturbing forces must produce greater effects; but the difference of the horizontal intensities at the Hague and Catania is very small in comparison with the inequalities observed; and it is therefore certain that the energy of the disturbing force was weaker the further we follow its action towards the south. With all the uncertainty under which we labour with respect to the nature of such disturbing forces, we cannot doubt that they have some definite source in space; and, as we must necessarily suppose those which produced the above-mentioned phænomena to have their seat to the north or to the north-west of the places of observation, (without venturing to define more precisely from so few data,) the northern districts, as far as we may venture to draw any such conclusions from experiments which embrace but a comparatively small portion of the earth's surface, appear to be the great focus, from whence proceed the greatest and most powerful actions.

A closer inspection of the data hitherto collected leads us to recognise, in the different successive movements, considerable variations in respect to their proportional magnitudes at different places, even when the similarity in other respects is unequivocal: thus, for instance, at one place, the first of two movements, following one shortly after the other, is the largest; at another place the reverse happens. We are therefore compelled to admit that, on the same day, and in the same hour, various forces are contemporaneously in action, which are probably quite independent of one another, and have very different sources; and the effects of these various forces are intermixed, in very dissimilar proportions, at various places of observation, relatively to the position and distance of these latter; or these effects may pass one into the other, one beginning to act before the other has

ceased. The disentanglement of the complications which thus occur in the phænomena at every individual station, will undoubtedly prove very difficult; nevertheless, we may confidently hope that these difficulties will not always remain insuperable, when the simultaneous observations shall be much more widely extended. It will be a triumph of science, should we at some future time succeed in arranging the manifold intricacies of the phænomena,—in separating the individual forces of which they are the compound result,—and in assigning the source and measure of each. Now and then we find at some places a small change, without any apparent counterpart at any of the other stations. Such occurrences ought not to be at once looked upon as evidences of local magnetic action. In so great a mass of numbers an error may sometimes take place. Cases have frequently occurred to us where a revision of the original observations, when these were in our hands, has shewn an error of calculation in the reduction, or an evidently accidental error in the writing. In other cases, in which we had received only an extract of the observations, a reference to our correspondent has led to a similar conclusion. As, however, it is impracticable to discuss all such cases by correspondence, those observers who do not communicate the original observations are requested, when they discover such cases in the curves representing them (as, for instance, at Leipzig, on the 26th of November, 1836, for 6^h 15^m Göttingen mean time), to refer to the original register. If errors are thus discovered, they can be corrected in a following number. Even when the original papers do not decidedly indicate any error, yet we cannot have perfect assurance with respect to cases which rest only on a single set of observations: it may happen, even to a practised observer, to write down in the same set repeated erroneous decimals. By such a conjecture, (somewhat hazarded it is true,) the above-mentioned number 11·69 would be reduced to 6·69, and thus correspond with the others.

But supposing the case of such an insulated movement to be established beyond all doubt, it does not follow that it is to be considered as local in the most limited sense. As the source of every anomaly must have its seat somewhere, it may be that the disturbing force is in the neighbourhood of the station itself. If feeble, its action may still be perceptible at that station, on account of its proximity, and may disappear (*i. e.* be no longer

perceptible) at all the other places of observations, because they are too remote. It appears, therefore, at least for the present, that there is no reason for admitting among the anomalies other than quantitative differences. Connected with this, it may be very useful, in many cases, to have two or more stations situated within a moderate distance of each other.

It would have been desirable, for example, to have had observations during the September term of 1836, at Augsburg, where the simultaneous observations are now regularly made. We should, in that case, have been able to form a decided opinion on the subject of the movement at $2^h\ 10^m$, everywhere sensible indeed, but which, at Munich, appears to have been of remarkable magnitude.

Note.

In the original work the observations made at the different stations in the several terms are printed in tables, and graphical representations of them are contained in six Plates. Much care has been taken to make the plates which are annexed to this translation faithful copies of the originals. It has not been thought necessary to republish the tables.—Edit.

Article III.

On the Combinations of Ammonia with Carbonic Acid. By Heinrich Rose, *Professor of Chemistry in the University of Berlin.**

[From Poggendorff's *Annalen*, vol. xlvi., part 3.]

An accurate examination of the combinations of ammonia with carbonic acid appeared to me to be important in several respects. Since the ultimate component parts of these combinations are exactly the same as those of animal substances, it was reasonable to suppose that they might easily combine in other proportions, and form new or already known combinations. It also appeared to me of importance to become acquainted with the properties of the anhydrous carbonate of ammonia, so as to be able to compare it with the other anhydrous salts of ammonia.

The examination, however, of these combinations has not afforded such results as I had expected. Carbonic acid and ammonia seem to belong to the last combinations into which substances containing oxygen, hydrogen, carbon, and nitrogen, become converted; and if therefore such bodies produce during their decomposition, by means of increased temperature, carbonate of ammonia, it is because the atoms of the elements in the carbonate of ammonia produced, are united in such a manner as to form combinations which are less easily decomposed, and, as it were, more stable, than the combinations consisting of these simple bodies generally are†. Neither did I find that the anhydrous carbonate of ammonia possessed any remarkable properties analogous to those by which the *anhydrous* is distinguished from the *hydrous* sulphate of ammonia. However, I discovered in my experiments on the combinations of ammonia with carbonic acid a fact which to me appeared worthy of attention; for, although these combinations are less decomposable than other bodies which consist of the same elements, yet carbonic acid and ammonia have little affinity for one another, and this is the rea-

* Translated by Mr. William Francis.

† Something similar occurs with grape sugar. A great number of organic substances are convertible by the action of very dilute acids into grape sugar, a substance scarcely decomposable, at least by weak acids.

son why they can both combine in the most varied proportions. The number of these combinations is in fact surprising. I have prepared several of them, the existence of which was previously unknown. It would, however, have been easy for me to have greatly increased their number by further examination, but I have contented myself with indicating the possibility of the existence of a great number of such combinations, since their preparation and examination would occasion more trouble than the subject appeared worthy of.

The reason of the great number of these combinations arises less from the weak affinity which carbonic acid has for ammonia, than from the circumstance that the various combinations have a great tendency to form double salts with each other. I have attempted to consider several salts which carbonic acid forms with ammonia as double salts combined in certain proportions, by which the number of the more simple combinations is limited.

Hitherto we were acquainted with only the following combinations of carbonic acid with ammonia in a solid state: 1. the anhydrous neutral carbonate of ammonia, $\underline{NH}^3 + \ddot{C}$; 2. the sesquicarbonate of ammonia, $2\ \underline{NH}^3 + 3\ \ddot{C} + 2\ \dot{\underline{H}}$, or rather the sesquicarbonate of the oxide of ammonium, $2\ \dot{\overline{NH}}^4 + 3\ \ddot{C}$; and 3. the bicarbonate of ammonia, $\underline{NH}^3 + 2\ \ddot{C} + 2\ \dot{\underline{H}}$, or $\dot{\underline{NH}}^4 + 2\ \ddot{C} + \dot{\underline{H}}$.

With respect to the analysis of the combinations of the carbonic acid with ammonia, the proportions of the ammonia and carbonic acid were determined directly, the water by the loss.

The determination of the ammonia may be effected with the greatest accuracy. The carbonate of ammonia was placed in a vessel which could be closed with a stopper, and a mixture of equal parts of muriatic acid and alcohol added; after the complete disengagement of all the carbonic acid, the solution was diluted by the addition of very strong alcohol (90 – 95 p. c.). Upon this an excess of a solution of chloride of platina, and then æther to nearly the amount of one fourth the volume of the alcohol, was added. The ammonio-chloride of platina is quite insoluble in a mixture of strong alcohol and æther, and may be collected without loss. I let it completely settle at the bottom of the stoppered bottle for twelve hours, and washed it out with a mixture of alcohol and æther. After desiccation it was cautiously exposed to ignition in a platina crucible. The salt was placed with the filtering paper in the crucible, and not as is usually done

with other precipitates, which are to be heated, taken out of the filter; the crucible was then closely covered with the lid, and exposed for a long time to a moderate heat, which was gradually raised to redness. This was continued until all the muriate of ammonia had evaporated. The crucible was then left to cool, the lid taken partly off, and the coal of the paper reduced in the usual way to perfect ash. If this precaution is not taken, and if the salt is heated too much at first, some undecomposed salt and metallic platina may be mechanically carried away with the vapours of the muriate of ammonia. From the weight of the metallic platina the ammonia in the salt is deduced. The determination of the ammonia may be effected in this way with great accuracy, much more so indeed than that of the carbonic acid.

The determination of the carbonic acid may be effected by two methods. The most accurate is to convert it into carbonate of barytes, and to determine from the weight of this salt that of the carbonic acid. The carbonate of ammonia was dissolved in cold water in a bottle, which could be closed air-tight, and a solution of chloride of barium was added to it; if the combination under examination does not consist of the neutral carbonate of ammonia, some pure liquid ammonia is added, after which the flask is closed, and left to stand at least for twelve hours, or longer. Care must be taken not to use too little water for the solution, and especially to test the ammoniacal fluid by a solution of the chloride of barium, to see whether it is free from carbonic acid. I have usually distilled the ammonia previous to the experiment over quick lime. The liquid, after the twelve hours' repose, was then passed through a filter, during which communication with the air was avoided as much as possible, and boiling water being poured upon the carbonate of barytes, it was filtered with the exclusion of air. This was frequently, and for a long time, washed with boiling water, but not until the water that passed through was no longer rendered milky by sulphuric acid, the residue not being quite insoluble in water. Repeated trials can alone determine when it is time to leave off the washing. The carbonate of barytes must not be filtered for some hours after precipitation, and until it has entirely settled. If it be filtered sooner, carbonate of barytes is deposited from the clear filtered liquid, even when no communication with the air has taken place.

When dry, the carbonate of barytes is heated. There is no need to fear, that by burning the filter any of the carbonic acid

of the carbonate of barytes is expelled; the strongest heat that an alcohol lamp, with double current of air, is capable of producing, may be applied without occasioning any loss.

A solution of chloride of calcium cannot be employed with the same advantage as one of chloride of barium for precipitating the carbonic acid. The carbonate of lime, it is true, does not form so bulky a precipitate as the carbonate of barytes; but a portion of the precipitate adheres so firmly to the sides of the vessel, that it is impossible to separate it completely by mechanical means. The heating of the carbonate of lime has also its disadvantages, as it then loses a portion of the carbonic acid.

In determining the carbonic acid in the neutral combinations, it was precipitated by chloride of barium without any addition of ammonia. In this case also the whole must be left to stand for some time after precipitation before the carbonate of barytes is filtered. When the solution of the neutral carbonate of ammonia is very weak, no precipitate is produced for some time by the chloride of barium, which is characteristic of the neutral salt. The liquid separated from the carbonate of barytes is then saturated with ammonia, in order to see whether any small precipitate would follow, which in general was the case; it was occasioned by the impossibility of obtaining the carbonate of ammonia always perfectly neutral. This precipitate, although filtered perfectly without exposure to the air, was nevertheless always more considerable than it should have been, and the amount of carbonic acid in the salt thus appeared to be greater than it really was.

The second method of determining the carbonic acid was by measuring it in the state of gas. A weighed quantity of the salt was decomposed in a graduated cylinder under mercury by means of muriatic acid, in which shortly previous to the experiment some carbonate of ammonia had been dissolved, in order to saturate it with carbonic acid. When the salt could only be employed in the form of powder, it was wrapped up in bibulous paper. This method, however, gave, even when all circumstances had been most carefully taken into consideration, less accurate results than by means of the carbonate of barytes. In general I obtained somewhat less carbonic acid than I ought. As, however, it is more quickly and easily performed, I have chiefly made use of it to ascertain to what known combinations of ammonia and carbonic acid any salt might belong.

1. *The Neutral Anhydrous Carbonate of Ammonia.*

It is well known that the neutral carbonate of ammonia is obtained by mixing dry ammoniacal gas with carbonic acid gas, and that both gases combine slowly, and only, (whichsoever of the two may be present in excess) as Gay-Lussac* first discovered, in the proportion of one volume of carbonic acid gas to two of ammoniacal gas. The properties of the anhydrous carbonate of ammonia obtained in this way are nevertheless almost unknown.

Dr. John Davy†, who last experimented on the combinations of ammonia with carbonic acid, confirmed the previous experiments of Gay-Lussac, without however subjecting the combination to a more accurate examination. He states that it possessed the property of being decomposed without effervescence by a neutral solution of chloride of calcium, and formed with it a neutral fluid.

I have only repeated the experiments of Gay-Lussac with the intention of learning whether, with an excess of ammoniacal gas, the two gases combined in the proportions above mentioned. I conveyed the carbonic gas into an excess of ammoniacal gas, and obtained the following results :

1. 29·7 vol. carbonic acid gas,	combined with	61	vol. of ammoniacal gas
2. 24·9	. . . . . .	49·75	———
3. 20·1	. . . . . .	38·15	———

The small differences are easily explained, by what I have on another occasion mentioned respecting the mixture of two gases which combine to form a solid body‡. The volume of the absorbed carbonic acid gas in the first experiment is evidently smaller on this account than it should be, because the carbonic acid gas was mixed with too great an excess of ammoniacal gas; in the second this was less, and in the third experiment still less.

Since the combination of ammonia with the carbonic acid gas is formed very slowly, no vapour is observed when a glass rod moistened with ammonia is held over a carbonated alkali, from which the carbonic acid is disengaged by sulphuric acid, as is always the case when volatile acids, such as muriatic acid, sulphurous acid, nitric acid, acetic acid, &c. are disengaged from a fluid by sulphuric acid.

* *Mémoires de la Société d'Arcueil*, tom. ii. p. 211.
† *Edinburgh New Philosophical Journal*, vol. xvi. p. 245.
‡ Poggendorff's *Annalen*, vol. xlii. p. 417.

To obtain the combination in great quantities, considerable portions of the dried gases were brought into contact, in large vessels, which had been filled with dry air. The combination adheres so firmly to the sides of the vessels, especially when they have been artificially cooled externally, that it is frequently possible to obtain it in no other way than by breaking them. It is only when no external refrigeration has been applied to the vessels that a portion of the combination can be obtained in a pulverulent state. I therefore subsequently caused the two dried gases to pass through several glass tubes, which were kept cool at their outer surface, in order to obtain larger quantities of the neutral salt. These tubes were then cut, and the salt deposited in them taken quickly out. On preparing the salt in this way, it was observed that on the combination of the two gases a very considerable increase in temperature takes place.

If in the preparation of this combination the greatest care is not taken to avoid every trace of moisture, which, with great quantities, it is very frequently difficult to effect, small admixtures of the hydrous combinations of ammonia occur with the anhydrous carbonate.

1·3425 gramme of the neutral anhydrous carbonate, dissolved in water, gave with a solution of chloride of barium 3·321 grm. of carbonate of barytes. The liquid filtered from it, treated with ammonia, gave a slight precipitate, the weight of which was not determined. 1·444 grm. of the combination, treated in the manner previously described with muriatic acid, alcohol, chloride of platina and æther, gave 3·461 grm. of heated metallic platina.

The carbonate of barytes obtained corresponds to 55·45 per cent. carbonic acid in the combination, and the quantity of platina to 41·69 per cent. ammonia. If we consider that a very small quantity of the hydrous bicarbonate of ammonia was contained in the combination, as would seem from a precipitate, although an inconsiderable one, being produced in the solution precipitated by chloride of barium and filtered, the compound formed agrees with the calculated formula $\underline{N}H^3 + \ddot{C}$, according to which 56·31 per cent. carbonic acid is combined in the salt with 43·69 per cent. ammonia.

It results from this examination, that in the solution of the anhydrous carbonate of ammonia, its constituents can be quantitatively separated by the same re-agents as in the case of the solutions of the hydrous combinations of ammonia with carbonic

acid. The anhydrous carbonate of ammonia is therefore differently circumstanced in this respect from the anhydrous sulphate of ammonia, the constituent parts of which cannot be separated by the same re-agents as those producing this effect with the corresponding hydrous salts. Neither does the anhydrous carbonate of ammonia in solution differ in its action with all the other re-agents from the other salts of the carbonate of ammonia, only that (which arises from its composition) the carbonic acid of the anhydrous neutral salt is entirely precipitated by solutions of the chloride of barium, and of the chloride of calcium, whilst this takes place in the solution of the other known combinations of carbonic acid and ammonia only after the addition of ammonia.

The anhydrous carbonate of ammonia is very easily soluble in water. In the solid state it smells like free ammonia. This is peculiar to all the combinations of carbonic acid with ammonia; but the greater the quantity of carbonic acid they contain, the weaker is the ammoniacal odour. It is not perceptible at first in the recently prepared combinations with excess of carbonic acid, and not till they have been preserved in a vessel for some time unexposed to the air. In the combinations, with more carbonic acid than contained in the neutral salt, this peculiarity may be ascribed to the circumstance that they do not volatilize undecomposed; in the anhydrous neutral salt, however, this is not the case, for it may be sublimed without changing its composition.

The neutral carbonate of ammonia is exceedingly volatile, and probably the most so of all the combinations of ammonia with carbonic acid. If exposed to the air, it disappears entirely in a short time. When sublimed, a very powerful ammoniacal odour is diffused, which, however, entirely arises from the volatilization of the undecomposed salt. 0·569 grm. of the sublimed salt, treated with chloride of platina, gave, after heating the platina salt obtained, 1·4656 grm. of metallic platina. 0·930 grm. of a second quantity of the sublimed salt gave, with a solution of chloride of barium, 2·267 grm. carbonate of barytes. Ammonia still produced a slight precipitate of 0·046 grm. carbonate of barytes in the filtered liquid, corresponding to 1·11 per cent. of carbonic acid. The quantity of the platina answers to 44·79 per cent. of ammonia, and that of the carbonate of barytes to 54·64, or, rather, 55·75 per cent. of carbonic acid, whence it evidently results that the salt had undergone no change from sublimation.

Since the salt undergoes no change in its composition by sublimation, and volatilizes at a low temperature, it was easy to determine the specific gravity of its vapour. This was performed according to the well-known method of Dumas*. Two experiments gave the following results:

	Weight of the glass globe filled with atmospheric air.	Weight of the glass globe filled with vapour.	Temperature at the melting.	Temperature of the air.	Corrected state of the barometer.	Volume of the glass globe.	Atmospheric air neglected.	Specific gravity calculated.
	Grammes.	Grammes.			Millimetres.	Cub. centim.	Cub. centim.	
I.	62·408	62·099	176°·25C.	15° C.	759·8	602·75	0·75	0·9018
II.	61·6765	61·383	140	8·75	753·1	597	6·5	0·8936

$$\begin{aligned} \text{But one vol. carbonic acid} &= 1{\cdot}52400 \\ \text{Two vols. ammonia . .} &= 1{\cdot}18240 \\ \hline & \ 2{\cdot}70640 \end{aligned}$$

The calculated specific gravity of a volume of the vapour of the neutral carbonate of ammonia is consequently 0·90213, which coincides particularly well with the result of the first experiment, which was performed with great accuracy, and it agrees also pretty fairly with the second.

The gaseous constituents in the vapour of the carbonate of ammonia are consequently combined without condensation.

For the first experiment a salt was taken, as it had been obtained by the method above mentioned. For the second experiment, on the contrary, a sublimed salt was employed. It is hence evident, that, as already proved by the analysis of the sublimed salt, its composition does not undergo any change by being sublimed once or even twice.

These experiments, however, disagree with those of M. Bineau†, who alleges that he had observed that the gaseous product obtained by exposing the salt to heat retains its gaseous property at a temperature which is lower than that at which it is formed. But his statement of the specific gravity of the vapour coincides with the results obtained by me, although he determined it in an

* The calculation was made according to Poggendorff's formula (*Annalen*, vol. xli. p. 449), having regard to the circumstance, that the gas of carbonic acid gas is lighter than the atmospheric air, the P of the formula, consequently the entire last member (p. 453) was taken negatively.

† *Annales de Chimie et de Physique*, vol. lxvii. p. 240.

apparently very uncertain way, by keeping the salt for a long time in contact with a measured volume of atmospheric air and leaving it to evaporate in it; he then treated the few cubic centimetres of the mixture alternately with dry oxalic acid and with potash, and thus obtained the volume of the ammonia and of the carbonic acid*.

Although the solution of the anhydrous carbonate of ammonia does not act differently towards the re-agents from the hydrous combinations of ammonia with carbonic acid, yet the combination, in its solid state, is distinguished on account of the absence of water, by its action upon several substances, from the sesquicarbonate of ammonia.

If dry muriatic gas is passed over the anhydrous carbonate of ammonia, no action is perceptible in the cold, even when the gas is left for a long time in contact with the salt. But if, during the passing, the ammoniacal carbonate is heated at one spot only for a moment, it decomposes there, and the heat gradually diffuses itself through the whole combination; and when it ceases, there is no longer any carbonic acid combined with the ammonia, and it has changed, naturally, without any disengagement of water, into hydrochlorate of ammonia. The common sesquicarbonate of ammonia is decomposed, even in the cold, by muriatic gas, with disengagement of heat. Water is formed by a slow disengagement of the carbonic acid at the upper surface of the glass sphere in which the mixture is contained.

The anhydrous carbonate of ammonia is at first not at all affected by gaseous chlorine; after an action of several days only does it gradually change, without any formation of water, into muriate of ammonia, in which case carbonic acid and nitrogen gas must necessarily escape. No formation of chloride of nitrogen takes place. The common sesquicarbonate of ammonia gradually changes, with perceptible disengagement of water, into the muriate of ammonia. If the salt is employed in pieces, they decompose very slowly, and, when taken out of the apparatus, effervesce with acids. Even in this case no production of chloride of nitrogen could be observed; however, the experiment was not continued until the salt had completely decomposed. The external portion of the salt, and the fine powder, had become perfectly converted into muriate of ammonia, without having indi-

* *Annales de Chimie et de Physique*, vol. lxviii. p. 434.

cated any trace of chloride of nitrogen. When the anhydrous carbonate of ammonia is treated with dry sulphurous acid, it assumes, even in the cold, a pale yellowish colour. If it is heated in an atmosphere of sulphurous acid gas, it changes entirely into an orange sublimate of anhydrous sulphite of ammonia. The solution acts with acids, solution of the nitrate of silver, and solution of chloride of mercury, &c., quite in the same way as the solution of the anhydrous sulphite of ammonia, which has been directly prepared from dry ammoniacal gas and dry sulphurous gas*. When the common sesquicarbonate is treated with dry sulphurous gas, no change is perceptible in the cold. But if the salt be slightly heated in the sulphurous gas, a yellow sublimate of anhydrous sulphite of ammonia is produced on the first action of caloric; but if the heat is continued, a white sublimate of the usual hydrous sulphite of ammonia is formed. If the whole is left to cool, and then suddenly heated anew, the same phænomena occur, and this may be repeated three or four times in the same way. But at last only white sublimate of anhydrous sulphite of ammonia is apparent. This decomposition of the salt into anhydrous and hydrous sulphite of ammonia is very easily explained if we regard the sesquicarbonate as being composed of anhydrous carbonate and of hydrous bicarbonate of ammonia.

When the anhydrous carbonate of ammonia is treated in the cold with dry sulphuretted hydrogen gas, no effect is produced. On the application of heat sulphuret of ammonia is formed without any evolution of water. The sesquicarbonate is likewise not affected by sulphuretted hydrogen gas in the cold, and even when heated it changes with difficulty, and partially only, with production of water, into sulphuret of ammonia; the greatest portion of the salt, however, may be sublimed in sulphuretted hydrogen gas.

An important difference between the anhydrous neutral and the hydrous sesquicarbonate of ammonia, is manifested in their respective actions with anhydrous sulphuric acid. When the vapours of this acid are passed over some powdered sesquicarbonate, it is decomposed, even when kept cold by a refrigerating mixture, with effervescence and evolution of carbonic acid, and the common hydrous sulphate of ammonia is formed. The neu-

* Poggendorff's *Annalen*, vol. xxxiii. p. 235.

tral anhydrous carbonate of ammonia, on the contrary, loses, by the action of the vapour of the anhydrous sulphuric acid, its carbonic acid, without any effervescence, and is converted into anhydrous sulphate of ammonia.

The neutral carbonate of ammonia may be prepared from the common sesquicarbonate in various ways, but not in a dry state. There is no way of obtaining it crystallized from a solution. The solutions of all the combinations of ammonia with carbonic acid, which contain more carbonic acid than the neutral salt, lose, when heated, carbonic acid, and are converted into the neutral combination; while the solution of this latter, evaporated at the common temperature, (in vacuum either over sulphuric acid or hydrate of potash,) loses ammonia, and changes into supercarbonates.

When the solution of the sesqui- or bi-carbonate of ammonia is boiled for a short time, it acquires the property of being thrown down entirely by an excess of a solution of the chloride of barium, or the chloride of calcium; so that pure ammonia produces no precipitate in the liquid filtered from the carbonated earth, nor even an opalescence. In the solution, therefore, there is a neutral combination of carbonate of ammonia. If the boiling is continued, the salt volatilizes entirely from the solution.

M. Hünefeldt* has shown, that when solid sesquicarbonate is subjected to distillation along with alcohol, on the boiling of the alcohol the carbonic acid escapes as gas; a portion of the alcohol then passes over, upon which a sublimate of a solid salt volatilizes with the remainder of the alcohol, at first adhering to the neck of the retort, and finally passing into the receiver with the vapours of the alcohol: this salt is the neutral carbonate of ammonia. I have frequently repeated this experiment in various ways, and convinced myself of the correctness of the fact. If the sublimed salt is dissolved in water, the solution is completely precipitated by a solution of the chloride of barium or the chloride of calcium in excess, and in such manner that no milkiness is produced by an addition of ammonia to the solution filtered from the carbonated earth.

It is, however, impossible to dry the neutral salt moistened with alcohol without its changing in its composition and losing some ammonia. When I dried it as quickly as possible by

* *Journal für praktische Chemie*, vol. vii. p. 25

means of bibulous paper, and then precipitated the solution of the dried salt by chloride of barium, I obtained from 1·042 grm. of the salt only 1·714 grm. of carbonate of barytes, which only answers to 36·87 per cent. carbonic acid in the salt. When, however, some ammonia was added to the filtered liquid, and the precipitate formed protected from the action of the air, I obtained 0·688 grm. carbonate of barytes, corresponding to 14·80 per cent. of carbonic acid in the salt.

I then attempted to dry the neutral salt, by placing it immediately in vacuo over sulphuric acid. The salt, it is true, became dry, but was no longer perfectly neutral, for its solution gave, after precipitation by the chloride of barium, a precipitate with ammonia. It is, nevertheless, the best method of drying the salt without its composition being considerably affected.

When I attempted to desiccate the salt moistened by alcohol over a considerable quantity of the hydrate of potash in vacuo, it remained moist although I kept it for more than a week under the air pump. The hydrate of potash became, it is true, carbonate at its surface, but a great quantity of ammonia was evolved in the gaseous form during the pumping.

The moist salt was then placed in a basin filled with fused chloride of calcium, and this again put into a larger basin containing hydrate of potash, and the whole then quickly placed in a vacuum. The chloride of calcium became covered with carbonate of ammonia; the remaining portion of the salt was dry, but after desiccation was no longer neutral.

I obtained a similar result when I employed quick lime instead of the hydrate of potash. When I brought this, as was the case with chloride of calcium, warm into the vacuum with the moist salt, the greater portion of it volatilized and deposited itself on the chloride of calcium; the small quantity of the salt remaining was not neutral.

I then placed the moist salt with another combination of chlorine of easy solubility in alcohol in vacuo. I chose for this purpose pulverized bichloride of mercury. The carbonate of ammonia remained moist, but the chloride of mercury attracted some of it, and did not dissolve entirely in water, the solution being opalescent. I obtained a remarkable result when I placed the moist salt under the air pump with quick lime and the acetate of lead. The carbonate of ammonia volatilized sooner than the alcohol with which it was moistened; it combined with the ace-

tate of lead, forming a tumid, white pasty mass, which effervesced with acids, and dissolved in water, leaving carbonate of lead behind. The alcohol was left in the fluid state, and contained some, although very little, ammonia. For this, and most of the other experiments, the sesquicarbonate was distilled with anhydrous alcohol.

It appears to result from these experiments that the carbonate of ammonia combines with some salts, and that it has towards these, even when they are soluble in alcohol, a greater affinity than alcohol towards them. However, this affinity does not seem to be very considerable, and probably occurs only under peculiar circumstances, perhaps not without the presence of a trace of water or alcohol, or at the common pressure of the atmosphere. For when I placed some anhydrous fused chloride of calcium, and some fused acetate of soda, in bottles which contained anhydrous neutral carbonate of ammonia, which had been prepared from a mixture of the carbonic and the ammoniacal gases, none of it was absorbed by the fused salts, not even when they had been moistened with some alcohol or water. The same is the case with fused chloride of calcium, which absorbs none of the usual sesquicarbonate of ammonia, when both are placed together in vessels. If, therefore, under certain conditions, the carbonate of ammonia appears to combine with some salts, this affinity cannot be compared to that which pure ammonia exhibits towards a great number of salts.

The experiments above mentioned, of drying the neutral carbonate of ammonia moistened with alcohol, were modified in various ways, but I never succeeded in obtaining a dry, undecomposed salt. The result was either that the salt remained moist or volatilized previous to desiccation, or that when it did become dry the salt was no longer neutral.

If the sesquicarbonate is distilled in a similar manner with æther, the phænomena are nearly the same: a considerable evolution of carbonic acid gas takes place during the distillation of the æther, but a far smaller quantity of carbonate of ammonia escapes with the æther than with the alcohol. The sublimed mass is the same neutral salt as that obtained with alcohol, and like it cannot be obtained pure in a dry state.

The only method by which I succeeded in obtaining a dry neutral carbonate of ammonia, besides that of preparing it from a mixture of carbonic acid gas with ammoniacal gas,

was by the sublimation of a mixture of anhydrous sulphate of ammonia and carbonate of soda. If every trace of moisture is avoided, a product is obtained as pure as by the mixture of the gases.

The impossibility of combining the anhydrous neutral carbonate in any way with the quantity of water which is requisite to convert the ammonia into the oxide of ammonium is in so far a very remarkable circumstance, as the carbonate of ammonia dissolved in water exhibits quite the identical properties which the carbonate of the oxide of ammonium would present, and, moreover, does not differ essentially in its other relations from other combinations of carbonic acid with ammonia, in which the latter may be regarded as the oxide of ammonium. Berzelius's view of considering the ammoniacal salts, on account of their water, as salts of the oxide of ammonium, is so plausible, and has justly been adopted by so many chemists, that the composition and properties of the anhydrous carbonate of ammonia do not suffice to render this view less probable. It must, therefore, be regarded as a body of a peculiar kind, belonging, with respect to its composition, to a class with the anhydrous combinations of ammonia with sulphuric acid and sulphurous acid, which latter, however, essentially differ in their properties from the carbonate of ammonia, in so far as these ammoniacal salts vary considerably in their action upon re-agents from the corresponding salts of the oxide of ammonium, and indicate in the most evident manner the distinction between combinations of ammonia and those of the oxide of ammonium. The most important distinction which exists between the anhydrous neutral carbonate and the hydrous combinations of ammonia with carbonic acid, which contain more carbonic acid, is that the former may be sublimed undecomposed, which is not the case with the latter.

It must be here mentioned that I have also prepared some anhydrous combinations of ammonia with oxy-acids, which, dissolved in water, did not differ, in their properties, from their corresponding salts of the oxide of ammonium, and in this respect are analogous to the carbonate of ammonia.

II. *The Neutral Hydrous Carbonate of Ammonia.*

The experiments mentioned in the preceding section show that it is not possible to combine the neutral anhydrous carbonate

of ammonia with the quantity of water which exactly suffices to change the ammonia into the oxide of ammonium.

I was much surprised at obtaining a hydrous neutral carbonate of ammonia in an unexpected manner. For if the sesquicarbonate of ammonia of commerce is exposed in a retort to a very gentle heat, and if the neck of the retort is connected with a longish glass tube, the other end of which is immersed in mercury, a disengagement of pure carbonic acid gas is first perceived, and in that part of the glass tube furthest from the heated retort a crystalline salt is deposited, the solution of which, in water, is so entirely precipitated by a solution of the chloride of barium, or the chloride of calcium, that ammonia produces no opacity, or at least only a very slight one, in the liquid separated from the carbonate of the earth. This salt is the most volatile of the solid products, which are produced during the distillation of the sesquicarbonate; if a gentle heat is applied for some time to the retort, the salt melts, and other combinations are formed and sublimed, which will subsequently come under our notice.

If the sesquicarbonate is exposed to a stronger heat, but little of the neutral salt is produced. It is therefore necessary to apply a very gentle heat, and only to employ for examination the products which are deposited in the part furthest from the heated portion of the retort. When this is not carefully attended to, a mixture of other combinations is obtained.

A mixture of sal-ammoniac and carbonate of soda gives, when exposed to heat under similar circumstances, the same salt. With this distillation, at first only ammoniacal gas escapes, as will be subsequently shown.

1·609 grm. of the sublimate, treated after having been dissolved with the chloride of barium, gave 3·596 grm. of carbonate of barytes: and 0·860 grm. of the sublimate, prepared in the same way, gave, when treated in the manner above mentioned, with alcohol, æther, muriatic acid, and chloride of platina, 1·942 grm. of metallic platina. This corresponds to the following composition:

Carbonic acid	50·09
Ammonia	39·27
Water	10·64
	100·00

The composition of this salt is very remarkable; only half the

quantity of water necessary to convert the ammonia into the oxide of ammonium is present. A composition calculated according to the chemical formula $\ddot{C} + \underline{NH^3} + \frac{1}{2}\,\underline{\dot{H}}$, gives in the hundred,

Carbonic acid	50·52
Ammonia	39·20
Water	10·28
	100·00

On repeating the experiment I obtained from 1·420 grm., 3·288 grm. of platina, and from 0·390 grm., 0·837 grm. of carbonate of barytes; and after an addition of ammonia, also 0·059 grm. This answers to the following composition:

Carbonic acid	51·49
Ammonia	40·26
Water	8·25
	100·00

It sometimes happens that it is difficult to obtain the salt perfectly pure. That its solution is not entirely precipitated by a solution of chloride of barium, but that, after the precipitation, a slight precipitate is still produced by ammonia, is almost always the case even with the solution of the anhydrous neutral salt.

The hydrous neutral carbonate of ammonia can, without changing very essentially in its composition, be again sublimed. 1·552 grm. of the twice sublimed salt gave, treated in the manner above mentioned, 3·692 grm. of metallic platina; and 0·446 grm. by means of chloride of barium, 1·009 grm. of carbonate of barytes; a precipitate of 0·077 grm. was nevertheless produced by ammonia. This answers to 41·37 per cent. ammonia, 50·71 per cent. carbonic acid, and also 3·87 per cent. carbonic acid in the precipitate caused by ammonia. We see clearly, that this salt, by the double sublimation, had changed in a small degree into a combination containing more carbonic acid, though it remained doubtful whether this was in consequence of the renewed action of heat, or on account of the attraction of moisture.

I then sublimed the first sublimate which had been obtained from two pounds of the sesquicarbonate, not less than five times, in order to see whether, by this means, it might entirely lose its water, and change into an anhydrous salt. The renewed sublimations

were effected in such manner that only the most volatile sublimate of each operation was employed for the following sublimation: 0·619 grm. of the obtained product gave 1·441 grm. of metallic platina, and from 0·552 grm. 1·288 grm. of carbonate of barytes were obtained by the chloride of barium; the liquid filtered from it gave, with ammonia, 0·068 grm. more. This corresponds to 40·48 per cent. ammonia, and 52·30 per cent. carbonic acid; and the last precipitate obtained 2·76 per cent. carbonic acid. The salt then does not become anhydrous by frequent sublimation.

If we admit that the composition first obtained is the correct one, and that the other salts contained a slight mixture of a combination, with a larger proportion of carbonic acid, then in fact this composition must appear a very remarkable one, for it is not favourable to the ingenious hypothesis proposed by Berzelius, that ammonia is changed into the oxide of ammonium by the reception of 1 atom of water, and is thus converted into a base. I shall, however, subsequently endeavour to show that the neutral anhydrous carbonate of ammonia has great tendency to form double salts, especially with the bicarbonate of the oxide of ammonium. This tendency it appears to evince also towards the simple carbonate of the oxide of ammonium, which does not seem to exist independently in a solid state. The most probable view which we may therefore take of the composition of the neutral hydrous carbonate of ammonia is, that we should look upon it as a combination of the carbonate of ammonia with the carbonate of the oxide of ammonium, $(\dot{C} + \underline{NH}^3) + (\dot{C}\ \underline{N\dot{H}^4})$.

If the anhydrous neutral salt, obtained by the mixture of the two gases, is not well preserved and protected from moisture, it appears to change into the hydrous neutral combination. On analysing such a salt, which had been sublimed, I obtained from 1·259 grm., 2·929 grm. of metallic platina, and from 0·784 grm. 1·844 of carbonate of barytes. This answers to 40·46 per cent. ammonia, and 52·72 per cent. carbonic acid.

It is surprising that the formation of the neutral carbonate of ammonia, during the distillation of the common sesquicarbonate, or a mixture of sal-ammoniac and dry carbonate of soda, has escaped the attention of chemists. I must, however, remark, that John Davy mentions in his paper* that his brother had obtained, on exposing the sesquicarbonate to heat, a salt which

* Edinburgh New Philosophical Journal, vol. xvi. p. 257.

possessed a decided ammoniacal odour, deliquesced when exposed to the air, and, as he believed, contained more ammonia than the known combinations. John Davy confirmed this experiment, and adds that it is more volatile than the last, and that probably it is hydrous carbonate of ammonia. I did not find that the hydrous neutral salt deliquesced in the air; but the salt, it is true, becomes moist, and remains so if the distillation is continued for any length of time, and water passes over.

III. *The Sesquicarbonate of Ammonia.*

This is the salt which occurs in commerce. I have analysed it several times, and found that, if it had not effloresced at its surface from the action of the atmosphere, and had not changed into the bicarbonate, it generally had, but not always, the composition which R. Phillips has assigned to it. The analyses were performed with quantities which had been obtained from various manufactories.

2·143 grm. of salt gave 3·530 grm. of metallic platina; 1·113 grm., however, of another quantity, 1·965 grm. of platina. The first quantity answers to 28·66 per cent., and the last to 30·70 per cent. of ammonia. The quantities of carbonic acid, which were determined in the gaseous form by means of muriatic acid over mercury, varied quite as much.

0·607 grm. gave 155 cub. centim.; 1·480 grm., 399·44 cub. centim.; and 1·419 grm. of the salt, 403 cub. centim. carbonic acid gas. This answers to 50, 55, 53, 40, and 56·23 per cent. carbonic acid in the salt.

These differences are explained by the modes of preparing the salt. When it has been prepared directly by sublimation from carbonate of lime and sal-ammoniac, or from sulphate of ammonia, then it is sesquicarbonate of ammonia. When, however, it has been once more sublimed in the manufactory, probably in order to purify it, it has changed into $\frac{5}{4}$-carbonate of ammonia, of which we shall speak hereafter.

The calculated composition of the sesquicarbonate, according to the formula $3\,\ddot{C} + 2\,\underline{NH^3} + 2\,\dot{\underline{H}}$, is

Ammonia	28·92
Carbonic acid	55·91
Water	15·17
	100·00

We find that there is sometimes in commerce a salt that contains about 31 per cent. ammonia, 51 per cent. carbonic acid. This is $\frac{5}{4}$ of carbonate of ammonia.

The composition of the sesquicarbonate of ammonia is such that it may be conceived as a combination of anhydrous neutral salt, and hydrous bicarbonate of the oxide of ammonium $(\ddot{C} + \underline{N}H^3) + (2\,\ddot{C} + \underline{\dot{N}}H^4 + \underline{\dot{H}})$; or if it is thought that the anhydrous neutral salt cannot exist in combination with hydrous salts of the oxide of ammonium, we might consider the formula $(\ddot{C} + \overline{\dot{N}}H^4) + (2\,\ddot{C} + \overline{\dot{N}}H^4)$ to be the more correct. Perhaps the preference might be given to the first formula, partly because the bicarbonate of the oxide of ammonium cannot be prepared alone, but is mixed with water, and at least with 1 atom of water; and partly because, as will be shown hereafter, anhydrous neutral carbonate of ammonia is volatilized when exposed to the air from the sesquicarbonate, and leaves behind hydrous bicarbonate of the oxide of ammonium.

This view is, in a great measure, confirmed by some recent experiments of Scanlan, and some earlier ones of Dalton*. They found that if the sesquicarbonate of ammonia is treated at the usual temperature for several times with less water than is necessary to dissolve it completely, the first saturated solutions had a greater specific weight than the last. In the same degree that the specific gravity of the solutions decreased, they lost their ammoniacal odour; the last solution gave crystals of the bicarbonate. They hence concluded, that either the sesquicarbonate is a mixture of two salts, or that the water exerts an action upon the salt similar to that it is usually imagined to have on some salts of bismuth, and that it decomposes it into two salts of two dissimilar degrees of saturation. Should, however, the last action take place, the salt of more difficult solution would remain in the form of a powder, which is not the case, for it is left as a skeleton.

The crystalline structure of the salt evidently shows that it is not a mere mixture, but is composed according to fixed proportions, which is also confirmed by analysis. But the experiments above mentioned prove that it is a double salt composed of 1 atom of neutral, and 1 atom of the bicarbonate of ammonia, both which constituents may be separated by water, according to their solubility in it. This separation, from the two salts being perfectly

* The Athenæum, 1838, No. 565, p. 596.

soluble, never more than approximates. When I, in the manner already mentioned, poured a little water upon the sesquicarbonate, I could not manage to obtain pure carbonate without a small mixture of dissolved bicarbonate; for, as I precipitated the solution with a solution of the chloride of barium, the filtrated liquid was rendered opalescent by ammonia.

The affinity between the two constituents in a double salt varies. The carbonate of ammonia is combined so feebly with the bicarbonate in the sesquicarbonate of ammonia, that water alone may cause a separation of both constituents. We find something similar in several double salts which are composed of a salt difficult, and of one easy of solution. Of the Brogniarti (Glauberit), a crystalline double salt of sulphate of lime and of sulphate of soda, the latter dissolves in water and leaves the sulphate of lime undissolved. In the same manner, according to Stromeyer, sulphate of potash and sulphate of magnesia is dissolved from the polyhallit of Ischl, by water, whilst sulphate of lime is left. According to Bauer, from the artificially prepared combination of carbonate of potash and carbonate of lime, water dissolves the first salt and leaves the last undissolved*; whilst, according to Boussingault, the Gaylussite, occurring in nature, which is similarly composed, withstands the action of the water, and is only easily decomposed by it when it has lost its water by being heated†.

Most of the other double salts, likewise composed of a salt easy and of one difficult of solution, are not at all decomposed by water. Common alum dissolves equally in water, without the readily soluble sulphate of alumina being separated by it from the sulphate of potash, which is of more difficult solution. The bisulphate of potash, which must be considered as a double salt, consisting of sulphate of potash and hydrate of sulphuric acid, acts in a similar way towards water; it also dissolves in water without decomposition. But between the two examples of double salts there is this difference, that, from the last salt alcohol separates the insoluble sulphate of potash, and dissolves the hydrate of sulphuric acid, whilst the alum resists the decomposition by aqueous alcohol, though the sulphate of alumina is soluble, and the sulphate of potash insoluble, in it.

The carbonate in the sesquicarbonate of the ammonia can

* Poggendorff's *Annalen*, vol. xxiv. p. 367. † Ibid. vol. vii. p. 99.

also be separated from the bicarbonate, not only by water, but also by being preserved in vessels from which the air is not entirely excluded. The more volatile carbonate gradually disappears entirely, and the less volatile bicarbonate is left quite free from the carbonate. This succeeds especially well if the sesquicarbonate is employed pulverized in the way above mentioned, and if the atmosphere in which the vessel is situated be not too moist. The remaining bicarbonate of the oxide of ammonium contains 1 atom of water; the volatile carbonate of ammonia is consequently anhydrous, and contains no oxide of ammonium, on which account, as remarked above, the latter can hardly be considered to exist in the common sesquicarbonate.

The double salts, which the carbonate of ammonia forms with the bicarbonate, are, however, in so far of an uncommon kind, that whereas in general the simple salts which form the constituents in other double salts are of one and the same degree of saturation, this is not the case here. We must, however, certainly distinguish two kinds of double salts. In the double salts of one kind, which form the majority, the simple salts are of the same degree of saturation; in them, generally half, or another definite portion of one base is replaced by an equivalent of another base, and the one salt consequently cannot act in them the part of an acid or a base towards the other, which was formerly the view taken with regard to the composition of these combinations. In the second kind of the double salts, on the contrary, both the combinations of which they consist are not of the same degree of saturation; in these double combinations one constituent part may be considered as the acid, the other as the base. Certain combinations of carbonic acid, of silicic acid, and of other weak acids with bases, belong to this class; and also the property of boracic acid to dissolve, when melted, all substances of acid and basic properties, depends on the tendency to form double salts of this second class.

In the combinations of the carbonate and of the bicarbonate of ammonia, which also belong to this class of double combinations, the carbonate is naturally the base, and the bicarbonate the part which replaces the acid. The tendency which the carbonate has to form a double salt with the bicarbonate, when sal-ammoniac or sulphate of ammonia is subjected with the carbonate of l me or a dry carbonated alkali to distillation, rests in part on this circumstance; that the carbonate of the oxide of ammonium, $\ddot{C}$ +

$N\dot{H}^4$, which ought here to be formed, does not seem to exist in a solid state of itself, as has already been remarked. On this account, at the beginning of the heating, ammonia is disengaged, and this escapes, in common, with so much water as would be necessary to convert it into the oxide of ammonium, whilst the sesquicarbonate of ammonia is formed. From 3 atoms of carbonate of oxide of ammonium, which ought to evolve from the mixture when heated, 1 atom of carbonate of ammonia is formed, and 1 atom of hydrous bicarbonate, which two form the double salt, and it disengages 1 atom of ammonia and 1 of water. $3\,\ddot{C} + 3\,NH^3 + 3\,\dot{H} = (\ddot{C} + NH^3) + (2\,\ddot{C} + N\dot{H}^4 + \dot{H}) + NH^3 + \dot{H}$. If the products of this operation are received in the order in which they are produced, over mercury, pure ammoniacal gas is first obtained, which is wholly absorbed by muriatic acid; and afterwards come the products, which appear during the sublimation of the common sesquicarbonate, of which we shall speak further on. As the sesquicarbonate can be evaporated only with the disengagement of carbonic acid gas, this gas is found amongst the products of the sublimation; there is, however a definite interval between the disengagement of the ammoniacal gas and of the carbonic acid gas. The latter first begins to escape when the evolution of the ammonia has entirely ceased, and when the glass cylinder, in which the gaseous products are received, begins to be covered with a thin incrustation of the carbonate of ammonia, and at the same time water passes over. When all the gaseous products are received together in one glass cylinder, over mercury, the ammoniacal gas which first goes over gradually combines with the carbonic acid gas which subsequently passes over.

IV. *Sesquicarbonate of Ammonia with a larger proportion of Water.*

If the common sesquicarbonate is exposed for some time to a very gentle heat, in a retort, the neck of which is connected with a long glass tube, the following appearances occur : at the very beginning carbonic acid gas is disengaged, and then the hydrous neutral carbonate of ammonia sublimes, which, as the most volatile of the solid products of sublimation, consolidates in that part of the glass tube furthest from the retort. The nearer to the retort the sublimate adheres, the more the solution is precipitated

by ammonia, after having been treated with chloride of barium, and the precipitated mass filtered.

The salt in the retort continually becomes moister, whilst the sublimate in the neck of the retort increases, and begins to be deposited in the body of the retort. At last a clear liquid only is left in the retort, from which, when the heat is over, a salt crystallizes, in the form of tables, in great quantity. The bulb of the retort must be broken, in order that the crystals may be well separated and obtained pure from the original mass. If the mass is preserved for a long time in closed vessels, a quantity of tables of the same salt is deposited from it, of more beautiful and distinct crystalline structure. This deposition of crystals continues for some weeks. When it ceases, the mass contains only neutral carbonate of ammonia in solution; by means of a solution of chloride of barium it is thrown down so completely, that ammonia produces no precipitate in the filtered liquid.

The salt sublimed in the neck and in the body of the retort, as well as that crystallized from the solution, are two combinations hitherto unknown. This sublimed salt will be treated of in the following section.

The crystals of the salt from the solution have the form of thin six-sided plates. On account of their thinness and rapid efflorescence the angles could not be measured. No cleavage could be observed.

Since this salt may be obtained in distinct crystals, it is consequently free from foreign mixtures; and the various analyses agree better with one another than is the case with those of the sublimed and non-crystalline combinations of carbonic acid with ammonia, and are more in unison with the calculated result. 1·904 grm. gave 2·594 grm. of metallic platina; 1·816 grm., with a solution of chloride of barium, treated with an addition of ammonia, gave 3·674 grm. of carbonate of barytes. This corresponds to the following composition:

Ammonia	23·69
Carbonic acid	45·35
Water	30·96
	100·00

0·714 grm. of the salt, treated with muriatic acid, gave 160·9 cub. centim. of carbonic acid gas; 0·598 grm. of the crystals, which were deposited in beautiful tables, after a long time, from the mother-liquor, gave, treated in a similar manner, 135 cub. centim. of carbonic acid gas. The first determination answers to 44·61 per cent.; the last to 44·69 per cent. of carbonic acid.

The proportion of the carbonic acid to the ammonia is the same as in the common sesquicarbonate; only the water is much greater. This salt corresponds to the combination $3\ddot{C} + 2\underline{NH^3} + 5\dot{\underline{H}}$, which, according to the calculation in the hundred, consists of,

Ammonia	23·56
Carbonic acid	45·55
Water	30·89
	100·00

Considered as a double salt, the composition would be represented by the formula $(\ddot{C} + \underline{NH^3}) + (2\ddot{C} + \underline{N\dot{H}^4}) + 4\dot{\underline{H}}$; or if we suppose the oxide of ammonium to be present in the carbonate, by $(\ddot{C} + \underline{N\dot{H}^4}) + (2\ddot{C} + \underline{N\dot{H}^4}) + 3\dot{\underline{H}}$.

The crystals do not retain their forms long when exposed to the air, but in closed glasses they do. Exposed to the air they lose water, and effloresce, and probably change in time, like the common sesquicarbonate, into the bicarbonate of ammonia.

V. *The five-four Carbonate of Ammonia.*

I have mentioned, that during the slow sublimation of the common sesquicarbonate in the neck and in the throat of the retort a sublimate is formed, which differs from that of the hydrous neutral salt, by its less degree of volatility, and also by its being formed in a much larger quantity. The sublimation must, however, be discontinued, when the contents of the retort have changed into a perfectly clear liquid in which no more solid salt can be perceived; if the heating be continued longer, much water passes over at the same time, and the sublimate changes in its composition.

The sublimate forms crystalline incrustations and masses. In its outward appearance it does not differ from the sesquicarbonate of commerce, but it does in its composition.

1·668 grm. of the salt gave, on experiment, 2·980 grm. of metallic platina; 3·496 grm. from the same specimen, treated with muriatic

acid over mercury, gave 934·5 cub. centim. of carbonic acid gas. The following composition is the result:

Ammonia	31·13
Carbonic acid	52·92
Water	15·95
	100·00

This corresponds to the following chemical composition: $5\ddot{C} + 4\underline{NH}^3 + 4\dot{H}$, or, $5\ddot{C} + 4\underline{N\dot{H}}^4$. The composition, calculated according to this formula, gives in the hundred,

Ammonia	31·85
Carbonic acid	51·38
Water	16·77
	100·00

Being obtained by sublimation, and not by crystallization from a solution, it is not of uniform composition. If the heating be further continued, so that considerably more water is evaporated, some of the salt described in the preceding section is obtained, which I shall have occasion subsequently to refer to. Besides, the quantity of this salt obtained is very considerable, being more than half of the sesquicarbonate employed, so that it is impossible that all specimens can be of the same purity. That it is not a mixture of several salts, but a distinct salt, is shown by its crystalline cleavage.

If we reflect on the composition of this salt, we shall immediately perceive that it may be regarded as a double salt of the common sesquicarbonate, and of the neutral carbonate of the oxide of ammonium, which cannot be prepared separately; for,

$$5\ddot{C} + 4\underline{N\dot{H}}^4 = (3\ddot{C} + 2\underline{N\dot{H}}^4) + 2(\ddot{C} + \underline{N\dot{H}}^4).$$

Since, however, as is evident from what precedes, it is highly probable that the sesquicarbonate itself must be considered as a double salt, it is far better to regard the salt as a combination of the carbonate of ammonia with the bicarbonate of the oxide of ammonium, the chemical formula of which would be

$$3(\ddot{C} + \underline{NH}^3) + (2\ddot{C} + \underline{N\dot{H}}^4 + 3\dot{H}).$$

The combination of the bicarbonate of the oxide of ammonium with 3 atoms of water, or of the bicarbonate of ammonia with 4 atoms of water, has not, it is true, been as yet prepared separately, but very probably it exists amongst the numerous combinations

of the bicarbonate with water, which might be prepared. If we suppose that the carbonate of ammonia in hydrous combinations must exist as carbonate of the oxide of ammonium, the formula will be $3(\ddot{C} + \underline{N\dot{H}^4}) + (2\ddot{C} + \underline{N\dot{H}^4})$, and then its water would exactly suffice to change all the ammonia into the oxide of ammonium.

I examined several different specimens of the sublimate obtained, and I sometimes obtained the expected results, and sometimes results somewhat differing. In some specimens the quantity of carbonic acid contained is more considerable, so that it appears to pass into the common sesquicarbonate.

1·151 grm. of the salt gave 2·094 grm. of metallic platina. This corresponds to 31·65 per cent. of ammonia in the salt.

1·433 grm. treated with a solution of chloride of barium and ammonia, gave 3·417 grm. carbonate of barytes; 1·852 grm., treated in the same manner, gave 4·517 grm. of carbonate of barytes. The first result corresponds to 53·44, the last to 54·66 per cent. of carbonic acid.

Treated with muriatic acid over mercury, I obtained,

0·496 gr.	132·66 C. C.	carb. acid gas,	corresponding to	52·95 p. c. carb. acid.
0·623	168·8	„	„	53·64
3·669	1000·8	„	„	54·00

It is seen that the quantity of carbonic acid in many specimens approaches very near to that in the common sesquicarbonate, which amounts to 55·9 per cent. On the other hand, I have already remarked, that I had found in common sesquicarbonate 30·70 per cent. instead of 28·92 per cent. of ammonia, and indeed, once only, 50·55 per cent. instead of 55·9 per cent. carbonic acid. The different quantities of the sesquicarbonate examined by me were purchased from manufactories. Their mode of preparation was, of course, unknown to me. It is probable that the specimens which approached in their composition to that of the $\frac{5}{4}$ carbonate of ammonia were obtained in the manufactories by repeated sublimation of the sesquicarbonate, perhaps with a view of purifying it. It is, however, also possible that they were produced by one very slow sublimation.

The sesquicarbonate is, moreover, converted entirely into this new salt, simply by the loss of half an atom of carbonic acid, which, during the sublimation, is constantly evolved in the form of gas. But as the hydrous neutral salt is formed at the same time, the process is not so simple. As that contains but little water, the

quantity of water in the portion left behind in the retort increases by its formation, so that sesquicarbonate and neutral salt might be obtained in solution from it; for I have mentioned above, that the sesquicarbonate, with a greater quantity of water, separates from the mother liquor which contains the neutral salt dissolved, which neutral salt is always formed with the disengagement of carbonic acid gas, when the solution of any combination of carbonic acid with ammonia, containing more carbonic acid than the neutral salt, is heated for a long while. The evolution of the carbonic gas during the slow sublimation of the sesquicarbonate is then caused through the formation, not only of the $\frac{5}{4}$ carbonate, but also of the neutral salt. If, however, it be admitted that the sesquicarbonate is a double salt of the carbonate and bicarbonate, it is then only the latter that loses a portion of its carbonic acid at the renewed sublimation. If the sublimation of the sesquicarbonate is further continued, till what is contained in the retort has changed into a clear liquid, the quantity of dissolved sesquicarbonate in it keeps decreasing, and at last the liquid consists of mere water, which only contains a very little dissolved carbonate of ammonia, and this is neutral. The sublimed salt in the neck and throat of the retort gradually dissolves in the hot water evaporated.

If this experiment be made with smaller quantities of common sesquicarbonate, or with mixtures of sal-ammoniac and dry carbonate of soda, heated in a small retort (the neck of which is connected with a long glass tube) gradually, but continued until the condensed water begins to show itself in the inclined glass tube, then, after having become perfectly cool in the neck of the retort, and in the part of the glass tube which is nearest to it, and in which no solid salt has sublimed, crystalline needles of a salt are deposited. I have often analysed this salt, but have obtained very different results in the several analyses. It seems usually to be the sesquicarbonate combined with the greater portion of water; for I obtained from 0·1235 grm., by a solution of chloride of barium and ammonia, 0·247 grm. of carbonate of barytes, which corresponds to 44·83 per cent. carbonic acid; 0·284 grm. of another quantity gave 0·387 grm. metallic platina, corresponding to 23·70 per cent. ammonia. But sometimes it is only the common sesquicarbonate, for in another experiment I obtained 1·582 grm. of the sublimate, 2·500 grm. of metallic platina, or 27·48 per cent. of ammonia. (Here I must remark that the

portion employed did not consist of pure crystals, but also in part of the solid sublimate). At times it is a mixture of both salts; for, from 0·189 grm. of the salt of another preparation I obtained 0·273 grm. of metallic platina, or 25·12 per cent. of ammonia.

The quicker the sublimation of the sesquicarbonate takes place, the less do the various products of the operation separate, and the more they are cooled the more they approach in their composition to the common sesquicarbonate. Under the conditions specified not so much carbonic acid is evolved in the form of gas as during the slow distillation, and it is again partly absorbed by the distilled salt, especially if kept well cooled. When I subjected common sesquicarbonate at a high temperature to a rapid sublimation, and cooled the product well, I obtained a moist salt, from which 1·246 grm. gave 1·903 grm. of metallic platina, which corresponds to 26·56 per cent. ammonia. Had the salt been dry, the quantity of ammonia would have corresponded still more nearly to that of the sesquicarbonate.

VI. *The five-four Carbonate of Ammonia, with a greater proportion of Water.*

If the $\frac{5}{4}$-carbonate of ammonia be subjected to a sublimation as slow as that of the sesquicarbonate; and if the operation be discontinued when the contents of the retort have changed into a clear liquid, and all the solid salt has disappeared, carbonic acid gas is again disengaged during the sublimation, and a solid salt is sublimed: the liquid in the retort, when perfectly cool, consolidates to an apparently solid salt, which, however, when laid between blotting paper, gives out much water. The disengagement of carbonic gas proves that simple carbonate has been again formed from one portion of the bicarbonate supposed to exist in the $\frac{5}{4}$ carbonate, which, as the most volatile product of sublimation, is deposited in the part of the tube furthest from the retort. On account of the insignificant quantities of the salt, however, which were employed on sublimation, it could not be obtained perfectly pure; and the solution of it, mixed with a solution of chloride of barium and filtered, was rendered somewhat opalescent by ammonia.

The salt sublimed in the neck of the retort, and in the receiver, have the same composition. It is the undecomposed

$\frac{5}{4}$ carbonate of ammonia, only with a greater quantity of water than the salt from which it was produced by sublimation. 0·9855 grm. of the salt gave 1·730 grm. of metallic platina, and 0·594 grm., treated with muriatic acid, 145·7 cub. centim. of carbonic acid gas. This gives the following composition:

Ammonia	30·53
Carbonic acid	48·56
Water	20·91
	100·00

The quantities of the salt examined were taken from the sublimate in the neck of the retort; 2·290 grm. of the sublimate in the receiver, treated with muriatic acid, gave 555·3 cub. centim. of carbonic acid gas; this corresponds to 48·01 per cent. of carbonic acid in the salt.

The composition found answers to the formula $5\ \ddot{C} + 4\ \underline{NH}^3 + 5\ \dot{H}$, which, calculated according to the same composition, is in the hundred,

Ammonia	30·61
Carbonic acid	49·32
Water	20·07
	100·00

Considered as a double salt of anhydrous carbonate, with hydrous bicarbonate, the composition of the salt might be expressed by the chemical formula $3\ (\ddot{C} + \underline{NH}^3) + (2\ \ddot{C} + \underline{N\dot{H}}^4 + 4\ \dot{H})$.

If we admit that the carbonate contains oxide of ammonium, the formula of the composition is $3\ (\ddot{C} + \underline{\dot{N}H}^4) + (2\ \ddot{C} + \underline{N\dot{H}}^4 + \dot{H})$. The salt, therefore, contains only 1 atom more water than that described in the preceding section from which it was prepared.

I obtained the same salt by the sublimation of the sesquicarbonate, with the greater proportion of water, $(3\ \ddot{C} + 2\ \underline{NH}^3 + 5\ \dot{H})$. During the operation much carbonic acid gas was evolved; 0·488 grm. of the sublimed salt gave 0·838 grm. of metallic platina; 0·643 grm., treated with muriatic acid, gave 163·9 cub. centim. of carbonic acid gas; at another experiment I obtained 0·578 grm., 150·8 cub. centim. This corresponds to 29·86 per cent. of ammonia, 50·46, and 51·65 per cent. of carbonic acid.

The salt, therefore, contained rather more carbonic acid than that obtained by sublimation from the $\frac{5}{4}$ carbonate of ammonia. The operation was continued until a clear liquid was left behind in the retort. On cooling, a salt crystallized, which, however, was not examined.

VII. *The five-four Carbonate of Ammonia with the greatest proportion of Water.*

The distillation of the $\frac{5}{4}$ carbonate of ammonia was continued until a clear fluid was left behind in the retort, which consolidated on cooling. The mass was pressed between bibulous paper, until it was no longer moist. When examined, it also proved to be $\frac{5}{4}$ carbonate of ammonia; but, with a very large quantity of water, 0·455 grm. of it gave 0·594 grm. of metallic platina. 0·524 grm., treated with muriatic acid, gave 101·403 cub. centim. of carbonic acid gas. This gives the following composition:

Ammonia	22·70
Carbonic acid	38·31
Water	38·99
	100·00

On a second examination, I obtained from 0·480 grm. 98·767 cub. centim. of carbonic acid gas, which correspond to 40·73 of carbonic acid. The composition answers to the formula,

$$5\,\ddot{C} + 4\,\underline{NH^3} + 12\,\underline{\dot{H}};$$

calculated according to this, the composition in the hundred is,

Ammonia	23·90
Carbonic acid	38·50
Water	37·60
	100·00

Considered as a double salt, the composition of the salt would be expressed by the formula,

$$3\,(\ddot{C} + \underline{NH^3}) + (2\,\ddot{C} + \underline{N\dot{H}^4}) + 11\,\underline{\dot{H}}.$$

If the ammonia is considered as the oxide of ammonium in both constituents, the formula of the salt is, $3\,(\ddot{C} + \underline{N\dot{H}^4}) + (2\,\ddot{C} + \underline{N\dot{H}^4}) + 8\,\underline{\dot{H}}$.

We see that the $\frac{4}{3}$ carbonate of ammonia is formed under the most varied circumstances. It cannot therefore be regarded as a mixture of the carbonate and bicarbonate; but it appears, like the sesquicarbonate, to be a true double salt.

VIII. *Bicarbonate of Ammonia.*

In works on chemistry it is stated that the bicarbonate of ammonia consists of 1 atom of ammonia, 2 atoms of carbonic acid, and 2 atoms of water. This is correct; but there is also a bicarbonate of ammonia, with a somewhat larger quantity of water, of which we shall speak in the following section. No chemical work treats of the last; yet it is very probable that it has often been produced, perhaps more frequently than the bicarbonate of the oxide of ammonium, with 1 atom of water, only it seems to have been confounded with it.

In my experiments I have obtained the salt $2\,\ddot{C} + \underline{NH}^3 + 2\,\dot{\underline{H}}$ in different ways, which I will here state.

I obtained this salt only once in large distinct crystals, as I evaporated a solution of the neutral carbonate of ammonia under the air-pump. I did not succeed again in producing a fresh quantity of such distinct crystals in a similar manner; but I still preserve those prepared at that time, after having analyzed a part of them. 1·281 grm. of it gave 1·576 grm. of metallic platina, and 1·458 grm., treated with a solution of chloride of barium and ammonia, gave 3·646 grm. of carbonate of barytes. This gives the following composition:

Ammonia	21·39
Carbonic acid	56·09
Water	22·52
	100·00

This corresponds well to the formula $2\,\ddot{C} + \underline{NH}^3 + 2\,\dot{\underline{H}}$, or rather $2\,\ddot{C} + \underline{\dot{N}H}^4 + \dot{\underline{H}}$; calculated accordingly, the composition in the hundred is,

Ammonia	21·60
Carbonic acid	55·72
Water	22·68
	100·00

This composition closely corresponds to that of the bicar-

bonate of potash, containing 1 atom of water ($2\ddot{C} + \dot{K} + \dot{\underline{H}}$), as 1 atom of the oxide of ammonium ($\underline{\dot{N}H^4}$) answers to an atom of potash; but the form also of the crystals of the bicarbonate of ammonia obtained is identical with those of the bicarbonate of potash. The crystals of the latter are, according to Levy*, a right oblique-angled prism of 103° 41′, and the crystals of the bicarbonate of ammonia have the same form.

I have prepared the same salt in another manner, but then in a pulverulent state.

If a perfectly saturated solution of the sesquicarbonate is evaporated over sulphuric acid under an air-pump, and the space over the solution kept as free from air as possible by pumping, it soon boils and deposits a salt difficult of solution. If it be taken out of the solution before it evaporates, and quickly dried between blotting paper, it appears, when analyzed, to be the bicarbonate of ammonia. 1·361 grm. gave 1·662 grm. of metallic platina; and 1·1135 grm., with a solution of chloride of barium and ammonia, 2·753 grm. of carbonate of barytes. This corresponds to the following composition:

Ammonia	21·24
Carbonic acid	55·42
Water	23·34
	100·00

If a saturated solution of the sesquicarbonate be slowly evaporated over sulphuric acid in vacuo, and if care be taken not to make it boil by too frequent pumping, another salt is obtained with more carbonic acid than in the bicarbonate, of which we shall speak in one of the following sections. If, however, instead of sulphuric acid, hydrate of potash and quick lime are employed for absorbing the water, a crystalline mass is obtained, which is the bicarbonate of ammonia. 1·168 grm. of it gave 1·418 grm. of metallic platina, and 0·959 grm., with a solution of chloride of barium and ammonia, 2·394 grm. of carbonate of barytes. This corresponds to the following composition:

Ammonia	21·12
Carbonic acid	55·95
Water	22·93
	100·00

* Quarterly Journal of Science, vol. xv. p. 286.

0·643 grm. of the salt prepared in this manner, and obtained on another occasion, gave with muriatic acid 182·25 cub. centim. of carbonic acid gas, which answers to 56·11 per cent. of carbonic acid in the salt.

The bicarbonate is likewise formed when, instead of the hydrate of potash and the quick lime, chloride of calcium is employed, and a saturated solution of the sesquicarbonate is evaporated with it in vacuo. 0·632 grm. of the salt obtained, treated with muriatic acid, gave 179·67 cub. centim. of carbonic acid gas, which corresponds to 56·28 per cent. of carbonic acid in the salt. The chloride of calcium did not smell of ammonia after the experiment, but it had taken up carbonate of ammonia, and, when dissolved, deposited carbonate of lime, whilst the muriate of ammonia was dissolved. If the crystals of the $\frac{3}{4}$ carbonate of ammonia (of which I shall speak in one of the following sections) are dissolved in water, and the solution is evaporated over sulphuric acid in vacuo, incrustations of the bicarbonate are obtained. 0·638 grm. gave 180·576 cub. centim. of carbonic acid gas, or 56·03 per cent. of carbonic acid.

In works on chemistry it is stated, that if the common sesquicarbonate be kept for a long time in vessels, in which it is not perfectly protected from the action of the air, it is gradually converted into bicarbonate. This statement is perfectly accurate. The more volatile carbonate flies off gradually, and the less volatile bicarbonate is left behind. I powdered some of the common sesquicarbonate, and kept it for five months in a dry room, in a vessel covered over with paper, which had been pricked with a needle. The powder was shaken from time to time. 2·021 grm. of the powder gave 2·510 grm. of metallic platina, and 0·728 grm., treated with muriatic acid, 205·5 cub. centim. of carbonic acid gas. This corresponds to

Ammonia	21·60
Carbonic acid	55·88
Water	22·52
	100·00

The composition of the bicarbonate of the oxide of ammonium, as well as that of the bicarbonate of potash, is quite analogous to that of the bisulphate of potash, and of the bisulphate of oxide of ammonium. These may, as is well known, be regarded as double salts of the sulphate of potash, or of oxide of ammonium, with hy-

drate of sulphuric acid; just so those might be regarded as double salts of the carbonate of potash and of oxide of ammonium, with hydrate of carbonic acid, if the latter were known in the isolated condition*. If this view could be maintained, and if, at the same time, it were to be admitted that hydrate of carbonic acid, and carbonate of ammonia ($\ddot{C} + \underline{NH^3}$) might replace one another, the formulæ of some double salts would turn out to be very simple. The neutral hydrous carbonate of ammonia, which is produced by the sublimation of the sesquicarbonate and other salts, would thus be analogous to the bicarbonate, only that the carbonate of ammonia of the former would be replaced in the latter by the hydrate of carbonic acid; both would be then united with the carbonate oxide of ammonium,

for the formula of the bicarbonate would then be $\ddot{C}\,\dot{H} + \ddot{C}\,\underline{N\dot{H}^4}$,

and that of the hydrous neutral carbonate of ammonia $\ddot{C}\,\underline{NH^3} + \ddot{C}\,\underline{N\dot{H}^4}$.

The common sesquicarbonate would then be a combination of carbonate oxide of ammonium, with carbonate of ammonia, and hydrate of carbonic acid. In general all combinations of carbonic acid and ammonia might then be considered as combinations of the carbonate oxide of ammonium, either with hydrate of carbonic acid, or with carbonate of ammonia, or with both; but some salts would still contain a superfluous quantity of water. In a table annexed to this memoir the compositions of all the salts described in this paper will be enumerated in formulæ, according to this view.

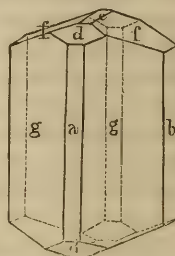
IX. *Bicarbonate of Ammonia, with a greater proportion of Water.*

If common sesquicarbonate be powdered, and as much boiling water poured upon it as is necessary to dissolve it; and if the glass, directly after the water has been added to it, is carefully closed, so that no carbonic acid gas, which is violently evolved from the sesquicarbonate by the boiling water, when the experiment is made in an open vessel, be lost, and is again absorbed by the solution as it cools; crystals of a large size are produced on the surface on cooling, which increase for many days. The greater the quantity of boiling water employed for the solution of the sesquicarbonate, the longer the time before

* It is perhaps possible that the solid carbonic acid, prepared by Thilorier, may be a hydrate of it.

they are produced, and the larger and more regular they are. By means of a very concentrated solution, we obtain them immediately on cooling, but often very small, and therefore difficult to be distinguished.

The violent disengagement of carbonic acid gas ceases directly if the glass be closed, as the gas is absorbed by the liquid through the pressure alone.



The crystals of this salt, which were examined by my brother, have the form of the annexed wood-cut. The planes *d* have, without the planes *a* and *c*, the form of rhomboids, therefore the planes *d f g* are the planes of three rhombic prisms, which render obtuse the three edges of one and the same rhombic octohedron; the planes *a b c* are the obtuse planes of three angles of this rhombic octohedron.

The inclination of	*g*	to	*g*	amounts to	112° 9′
„	*d*	„	*d*	„	118 33
„	*d*	„	*c*	„	149 16½
„	*d*	„	*a*	„	120 43½
„	*d*	„	*g*	„	115 5
„	*f*	„	*f*	„	136 25
„	*f*	„	*c*	„	158 12½
„	*f*	„	*b*	„	111 47½
„	*f*	„	*g*	„	101 56*

The planes are in general very smooth and bright, and are well adapted for accurate measurements, the plane *c* alone is generally somewhat rounded. The crystals are perfectly cleavable, parallel to the planes of the vertical prism *g*; no other surfaces of cleavage besides this were observed. The planes of the vertical prism are, in the various deposits, sometimes large, sometimes small; frequently so small that the planes of the superior and inferior extremity are in contact.

These crystals have undoubtedly been obtained before now; I produced them many years ago of considerable size, from a solution of a great quantity of the sesquicarbonate, for which

* Only the inclinations of *g* to *g* = 112° 9′, and *g* to *d* = 115° 5′, are the direct results of measurement; the other angles mentioned are calculated from these, but these also were measured, and the measured and calculated angles were mostly found to differ only by a few minutes.

hot water was employed. It seems to me that they have always been looked on as the common bicarbonate, from which they differ, not only by the form of the crystals, but also, as will be seen immediately, in their composition. It is, however, also possible that the bicarbonate, with 2 atoms of water, which was treated of in the preceding chapter, may never have been obtained crystallized; since, notwithstanding all my endeavours, I only succeeded once in producing distinct crystals, at other times only powder and crusts; and, that all the crystallized bicarbonate formerly obtained contained more water. The imperfect descriptions of the crystals given by Schrader* and others seems at least to render this view probable, although, on the other hand, all the analyses with which I am acquainted, give 55 to 56 per cent. carbonic acid in the bicarbonate, which agrees with the salt described in the preceding chapter.

2·722 grm. of the salt gave 3·179 grm. metallic platina, and 3·406 grm. of the same preparation, treated with muriatic acid, 890·66 cub. centim. carbonic acid gas.

1·802 grm. of the salt produced 2·043 grm. of metallic platina, and 1·116 grm., with a solution of the chloride of barium and ammonia, 2·630 grm. of carbonate of barytes. 0·764 grm. of the same salt afforded, with muriatic acid, 185·11 cub. centim. carbonic acid gas. These answer to the following compositions:

	I.	II.
Ammonia . .	20·31	19·72
Carbonic acid .	52·82	52·96 — 52·05
Water	26·87	27·32
	100·00	100·00

which corresponds to the chemical formula $4\,\ddot{C} + 2\,\underline{NH^3} + 5\,\dot{H}$, or rather $4\,\ddot{C} + 2\,\underline{N\dot{H}^4} + 3\,\underline{\dot{H}}$; calculated according to this, it would be in the hundred,

Ammonia	20·45
Carbonic acid	52·73
Water	26·82
	100·00

The salt therefore differs from the other bicarbonate only by its containing one half of an atom more water.

* *Neues allgem. Journal der Chemie*, vol. ii. p. 582.

X. *Bicarbonate of Ammonia, with greatest quantity of Water.*

By the distillation of the $\frac{5}{4}$ carbonate of ammonia, with 5 atoms of water, which was prepared by distilling the $\frac{5}{4}$ carbonate, with 4 atoms of water, I obtained a sublimate, which, on examination, proved to be a bicarbonate, with a still larger portion of water. 1·351 grm. of it gave 1·408 grm. of metallic platina; 0·681 grm., with muriatic acid, 174·3 cub. centim., and 0·512; in a second experiment 131·55 cub. centim. carbonic acid. This corresponds to the following composition:

Ammonia	18·12
Carbonic acid	50·67 — 50·86
Water	31·21
	100·00

which would be represented by $2\ddot{C} + \underline{NH^3} + 3\dot{\underline{H}}$, or rather $2\ddot{C} + \underline{N\dot{H}^4} + 2\dot{\underline{H}}$; and the salt, calculated according to this, would be composed of

Ammonia	19·41
Carbonic acid	50·05
Water	30·54
	100·00

As this salt was procured only in small quantity, its preparation should, by right, have been repeated in larger quantities. I have, however, thought it better to mention these experiments in this place, to show that the bicarbonate, like all the other combinations of carbonic acid and ammonia, is capable of combining with very different quantities of water.

XI. *Seven-four Carbonate of Ammonia.*

This salt was obtained by distilling the bicarbonate with a greater quantity of water, $4\ddot{C} + 2\underline{N\dot{H}^4} + 3\dot{\underline{H}}$, during which process a disengagement of carbonic acid gas took place, and, in fact, phænomena similar to those which occurred with the sublimation of the sesquicarbonate. 0·923 grm. of the sublimed salt gave 1·030 grm. metallic platina; 0·602 grm. produced 148·7 cub. centim., and 0·545 grm., 131·32 grm. of carbonic acid gas, giving the following composition:

Ammonia	19·41
Carbonic acid	47·70 — 48·90
Water	32·89
	100·00

which correspond best with the chemical formula $7\,\ddot{C} + 4\,\underline{NH^3} + 12\,\underline{\dot{H}}$, or rather, $7\,\ddot{C} + 4\,\underline{N\dot{H}^4} + 8\,\underline{\dot{H}}$. When the composition is calculated according to this we obtain in the hundred,

Ammonia	20·71
Carbonic acid	46·71
Water	32·58
	100·00

The preparation of this salt, like that of the bicarbonate with the greatest quantity of water, should be repeated; for the experiments with these two salts were the last which I made on the combinations of ammonia with carbonic acid, and, moreover, at a time when I had already resolved to discontinue these examinations.

I have, however, made especial mention of this salt, because, if it be regarded as a double salt, in which the anhydrous neutral carbonate forms the one constituent, we are then compelled to consider the other constituent as a combination of 4 atoms of carbonic acid, and 1 atom of ammonia or the oxide of ammonium,—a combination which has never yet been prepared isolated. According to this view the composition of the salt would be expressed by the formula $3\,(\ddot{C} + \underline{NH^3}) + (4\,\ddot{C} + \underline{NH^3} + 12\,\underline{\dot{H}})$, or rather, $3\,(\ddot{C} + \underline{NH^3}) + (4\,\ddot{C} + \underline{N\dot{H}^4} + 11\,\underline{\dot{H}})$, or $3\,(\ddot{C} + \underline{N\dot{H}^4}) + (4\,\ddot{C} + \underline{N\dot{H}^4} + 8\,\underline{\dot{H}})$.

XII. *Nine-four Carbonate of Ammonia.*

When a solution of the common sesquicarbonate is evaporated over sulphuric acid in vacuo, and too much pumping is avoided, so that the solution may not boil, small crystals are obtained, which, immediately on their formation, must be withdrawn from the influence of the sulphuric acid, otherwise they effloresce and pass into the common bicarbonate.

1·507 grm. of these crystals gave 1·657 of platina, and 1·176 grm. of crystals from the same preparation gave, treated with chloride of barium and ammonia, 2·929 grm. of carbonate of barytes, which gives the following composition:

Ammonia	19·12
Carbonic acid	55·83
Water	25·05
	100·00

This answers to the chemical formula $9\ddot{C} + 4\underline{NH^3} + 10\underline{\dot{H}}$, or, $9\ddot{C} + 4\underline{N\dot{H}^4} + 6\underline{\dot{H}}$. Calculated accordingly, the composition would be,

Ammonia	19·19
Carbonic acid	55·65
Water	25·16
	100·00

0·866 grm. of beautiful crystals prepared on another occasion gave 0·920 grm. metallic platina, which corresponds to 18·47 per cent. ammonia in the salt.

It is necessary to determine, in the analysis, the quantities of ammonia and carbonic acid from the same portion of crystals. I examined some transparent crystalline incrustation of a different portion, only for carbonic acid, as I obtained but a small quantity of them in a pure state: 0·664 grm. gave 192·29 cub. centim. carbonic acid gas by means of muriatic acid, which answers to 57·33 per cent. carbonic acid. Perhaps this crust might have been the $\frac{9}{4}$ carbonate of ammonia with a different quantity of water; for if we only suppose 9 atoms of water, instead of 10, in the salt, which, in fact, is a more probable amount of water, then it would contain,

Ammonia	19·68
Carbonic acid	57·09
Water	23·23
	100·00

The $\frac{9}{4}$ carbonate of ammonia is only produced by the action of sulphuric acid on a solution of the sesquicarbonate. Whilst it changes into the bicarbonate by the loss of neutral carbonate of ammonia, the ammonia alone of this last is absorbed by the sulphuric acid, the surface of which becomes covered with a strong efflorescence of sulphate oxide of ammonium, and the carbonic acid gradually combines with the bicarbonate. If the pumping is performed rather quickly bicarbonate only is formed, under the above-mentioned circumstances, from the sesquicarbonate, because the carbonic acid is then too rapidly removed. Also, on employing hydrate of potash, lime, or even chloride of calcium, instead of sulphuric acid, bicarbonate only is produced; the chloride of calcium absorbs the carbonate of

ammonia, and is not capable, even when employed in great quantity, of decomposing it, although pure ammonia combines easily, and in great quantity, with the chloride of calcium.

The $\frac{9}{4}$ carbonate of ammonia only feebly retains that portion of carbonic which it contains, more than is requisite to form the bicarbonate. It is only when the salt has separated in crystals that it resists speedy decomposition. But even in the solid state it loses the last portion of carbonic acid, and easily effloresces into the bicarbonate: on which account the crystals, when once formed, under the air-pump, must not be allowed to remain there. But the salt not only effloresces and passes into the bicarbonate, when exposed to the air, but even in the closed vessels in which it is intended to preserve it.

The preparation of this salt, moreover, is very uncertain, and I have succeeded but very rarely. It seems to depend on the concentration of the solution, and on the evaporation under the air-pump, which must neither be too quick nor too slow. If a saturated solution of the sesquicarbonate is evaporated over sulphuric acid, without being placed in vacuo, the whole of the carbon disappears with a slower evaporation of the water, and nothing is left behind.

If we regard the $\frac{9}{4}$ carbonate of ammonia as a double salt, we must admit the presence of the carbonate, together with the bicarbonate; and the quadricarbonate contained in the $\frac{7}{4}$ carbonate of ammonia, and the very compound formula would be $(\ddot{C} + \underline{NH^3}) + (4\,\ddot{C} + 2\,\underline{\dot{N}H^4}) + (4\,\ddot{C} + \underline{\dot{N}H^4}) + 7\,\underline{\dot{H}}$, or 6 $\underline{\dot{H}}$, if 9 atoms of water are contained in the salt.

It must be evident to every one that the number of combinations of carbonic acid with ammonia might easily have been increased had I continued further my experiments on this subject; for the sublimation of the various kinds of bicarbonate, of $\frac{7}{4}$, and of the various kinds of the $\frac{9}{4}$ carbonate of ammonia, would certainly have produced new double salts, which may be imagined as formed of the carbonate, united with bicarbonate and the quadricarbonate. I thought it best, however, after having contented myself with indicating the possibility of the existence of this great number of double salts, to discontinue the examination.

Among the combinations of carbonic acid with ammonia, it

is only the $\frac{9}{4}$ carbonate which loses a portion of the carbonic acid in its solid state; the other combinations, on the contrary, undergo at the ordinary temperature quite a reverse decomposition; they have a tendency to evolve partly ammonia, partly anhydrous carbonate, which have just the same odour as pure ammonia. When, therefore, the recently-prepared acid salts do not smell of ammonia, it nevertheless arises when they have been kept for some time in a well-closed vessel, if this be opened. Even the bicarbonate forms no exception to this; and this perhaps seems to indicate that it has a tendency to pass into the $\frac{9}{4}$ carbonate. The solution of the carbonates of ammonia have a tendency, at the common temperature, to smell of ammonia.

With an increased temperature, the solutions of the carbonates, as well as they themselves, the carbonate naturally excepted, evolve on the contrary carbonic acid. The solutions of all the carbonates are converted by boiling into neutral salt, and the solid salts lose a portion of the acid, and produce partly neutral, and partly less acid salts.

I think it will be advantageous to enumerate, in a table at the end of this memoir, the chemical formulæ of the combinations examined according to the various views which may be entertained with regard to their composition, so that they may easily be compared one with another.

		The Carbonic Acid supposed to be combined with Ammonia.	The Carbonic Acid supposed to be combined with the Oxide of Ammonia.	The combinations regarded as double salts of Anhydrous Carbonate, with Bicarbonate and Quadricarbonate.	The Salts regarded as combinations of the Neutral Carbonate Oxide of Ammonium, with Carbonate of Ammonia, and with the Hydrate of Carbonic Acid.
I.	Neutral Anhydrous Carbonate of Ammonia	$\ddot{C}+\underline{NH^3}$			
II.	Neutral Hydrous Carbonate of Ammonia	$\ddot{C}^2+2\underline{NH^3}+\underline{\dot{H}}$	$\ddot{C}\underline{NH^3}+\ddot{C}\underline{\dot{N}H^4}$	$\ddot{C}\underline{NH^3}+\ddot{C}\underline{\dot{N}H^4}$	$\ddot{C}\underline{\dot{N}H^4}+\ddot{C}\underline{NH^3}$
III.	$\frac{5}{4}$ Carbonate of Ammonia	$\ddot{C}^5+4\underline{NH^3}+4\underline{\dot{H}}$	$\ddot{C}^5+\underline{\dot{N}H^4}$	$3\ddot{C}\underline{NH^3}+\ddot{C}^2\underline{\dot{N}H^4}+3\underline{\dot{H}}$	$3\ddot{C}\underline{\dot{N}H^4}+\ddot{C}\underline{NH^3}+\dddot{C}\underline{H}$
IV.	$\frac{5}{4}$ Carbonate of Ammonia, with greater amount of Water........................	$\ddot{C}^5+4\underline{NH^3}+5\underline{\dot{H}}$	$\ddot{C}^5+4\underline{\dot{N}H^4}+\underline{\dot{H}}$	$3\ddot{C}\underline{NH^3}+\ddot{C}^2\underline{\dot{N}H^4}+4\underline{\dot{H}}$	$4\ddot{C}\underline{\dot{N}H^4}+\dddot{C}\underline{H}$
V.	$\frac{5}{4}$ Carbonate of Ammonia, with the greatest amount of Water	$\ddot{C}^5+4\underline{NH^3}+12\underline{\dot{H}}$	$\ddot{C}^5+4\underline{\dot{N}H^4}+8\underline{\dot{H}}$	$3\ddot{C}\underline{NH^3}+\ddot{C}^2\underline{\dot{N}H^4}+11\underline{\dot{H}}$	$4\ddot{C}\underline{\dot{N}H^4}+\dddot{C}\underline{H}+7\underline{\dot{H}}$
VI.	Sesquicarbonate of Ammonia............	$\ddot{C}^3+2\underline{NH^3}+2\underline{\dot{H}}$	$\ddot{C}^3+2\underline{\dot{N}H^4}$	$\ddot{C}\underline{NH^3}+\ddot{C}^2\underline{\dot{N}H^4}+\underline{\dot{H}}$	$\ddot{C}\underline{\dot{N}H^4}+\ddot{C}\underline{NH^3}+\dddot{C}\underline{H}$
VII.	Sesquicarbonate of Ammonia, with greater amount of Water	$\ddot{C}^3+2\underline{NH^3}+5\underline{\dot{H}}$	$\ddot{C}^3+2\underline{\dot{N}H^4}+3\underline{\dot{H}}$	$\ddot{C}\underline{NH^3}+\ddot{C}^2\underline{\dot{N}H^4}+4\underline{\dot{H}}$	$2\ddot{C}\underline{\dot{N}H^4}+\dddot{C}\underline{H}+2\underline{\dot{H}}$
VIII.	$\frac{7}{4}$ Carbonate of Ammonia	$\ddot{C}^7+4\underline{NH^3}+12\underline{\dot{H}}$	$\ddot{C}^7+4\underline{\dot{N}H^4}+8\underline{\dot{H}}$	$3\ddot{C}\underline{NH^3}+\ddot{C}^4\underline{\dot{N}H^4}+11\underline{\dot{H}}$	$4\ddot{C}\underline{\dot{N}H^4}+3\dddot{C}\underline{H}+5\underline{\dot{H}}$
IX.	Bicarbonate of Ammonia	$\ddot{C}^2+\underline{NH^3}+2\underline{\dot{H}}$	$\ddot{C}^2+\underline{\dot{N}H^4}+\underline{\dot{H}}$	$\ddot{C}^2\underline{\dot{N}H^4}+\underline{\dot{H}}$	$\ddot{C}\underline{\dot{N}H^4}+\dddot{C}\underline{H}$
X.	Bicarbonate of Ammonia, with greater amount of Water........................	$\ddot{C}^4+2\underline{NH^3}+5\underline{\dot{H}}$	$\ddot{C}^4+2\underline{\dot{N}H^4}+3\underline{\dot{H}}$	$2\ddot{C}^2\underline{\dot{N}H^4}+3\underline{\dot{H}}$	$2\ddot{C}\underline{\dot{N}H^4}+2\dddot{C}\underline{H}+\underline{\dot{H}}$
XI.	Bicarbonate of Ammonia, with the greatest amount of Water	$\ddot{C}^2+\underline{NH^3}+3\underline{\dot{H}}$	$\ddot{C}^2+\underline{\dot{N}H^4}+2\underline{\dot{H}}$	$\ddot{C}^2\underline{\dot{N}H^4}+2\underline{\dot{H}}$	$\ddot{C}\underline{\dot{N}H^4}+\dddot{C}\underline{H}+\underline{\dot{H}}$
XII.	$\frac{9}{4}$ Carbonate of Ammonia	$\ddot{C}^9+4\underline{NH^3}+10\underline{\dot{H}}$	$\ddot{C}^9+4\underline{\dot{N}H^4}+6\underline{\dot{H}}$	$\ddot{C}\underline{NH^3}+2\ddot{C}^2\underline{\dot{N}H^4}+\ddot{C}^4\underline{\dot{N}H^4}+7\underline{\dot{H}}$	$4\ddot{C}\underline{\dot{N}H^4}+5\dddot{C}\underline{H}+\underline{\dot{H}}$

I may be allowed to add a few additional remarks on the chemical formulæ in the last column. If the various salts of carbonic acid with ammonia be regarded as combinations of the carbonate oxide of ammonium, with carbonate of ammonia and the hydrate of carbonic acid, some of the salts will still contain superfluous water. This is the case with the 5th, 7th, 8th, 10th, 11th, and 12th; yet it is possible, as was previously noticed, that the latter salt may be prepared with 1 atom less of water, and then it would not belong to this section. I am inclined to consider this water as real water of crystallization; I have, however, not performed any experiments to ascertain whether it might be removed without any change in the composition. With respect to the view, that carbonate oxide of ammonium is partly combined with carbonate of ammonia, partly with the hydrate of carbonic acid, and partly with both together in the salts described, this is founded on a hypothesis, upon which I lay but little stress, and which needs more confirmatory facts before it can be adopted. In a memoir communicated many years ago to the *Annalen**, I endeavoured to show that in numerous salts ammonia acted quite the same part as the water of crystallization, and that it might, as it were, replace it. It may, therefore, be possible, that water, even when not existing as water of crystallization and ammonia, both combined with carbonic acid, might form bodies which might equally be replaced. This at least appears to be the case in those combinations which these bodies form with the carbonate oxide of ammonium. If we admit this view, several of the ammoniacal salts described would have, as was already noticed with respect to the hydrous neutral carbonate and the bicarbonate, an identical composition.

From a subsequent communication from M. Bauer, the artificially-prepared combination of carbonate of soda and carbonate of lime stand in the same relation to water as the Gay-Lussite which occurs in nature.

* Poggendorff's *Annalen*, vol. xx. p. 163.

Article IV.

Memoir on the Polarization of Heat; by Macedoine Melloni. Second Part*.

[From the *Annales de Chimie et de Physique*, vol. lxv., May, 1837.]

In the first part of this Memoir it was shown, that calorific rays, transmitted by a pair of tourmalines which completely polarize light, undergo every degree of polarization. Certain species of heat, in sensibly equal quantities, traverse the two plates, whatever be the position, parallel or perpendicular, of their axes of crystallization: others pass, in different proportions, in these two directions of the axes; and, lastly, others only traverse the system when the axes of crystallization are parallel.

In the examination of the method according to which the polarization of light becomes sensible by means of tourmalines, it was shown that, notwithstanding the great differences of effect presented by the various species of heat, it was not necessary to suppose the existence of a different aptitude for polarization in each; but, on the contrary, that all might undergo an equal and complete polarization in the interior of the tourmalines, and yet appear more or less polarized at their emergence. These effects are sufficiently accounted for by supposing that the tourmalines refract doubly every sort of radiant heat, and that, in each particular case, one of the two pencils of rays proceeding from this double refraction is more or less absorbed during its passage. The two refracted pencils being of the same intensity, polarized

* [The first part of this Memoir will be found in Scientific Memoirs, vol. i. p. 325.]

at right angles, and sensibly superposed, it might be expected that if they have undergone the same degree of absorption, no index of polarization would be presented by them when issuing from the plates; but if one of them has lost, in its passage, a greater portion of its intensity, the other will necessarily give signs of polarization on its emergence; and the appearances of this phænomenon will become exactly similar to those presented by light when one of the two refracted pencils is entirely absorbed in the interior of the plates.

According to this manner of considering the subject, the more or less absorbent action of the tourmalines upon one of the doubly refracted pencils of heat would enter, so to speak, into the class of facts that have been observed in our examination of simple calorific transmission by solid and liquid bodies, and all the rays of heat, like light of every colour, would be susceptible of complete polarization by the forces which produce reflection and refraction. This latter conclusion will appear, with the clearest evidence, from the numerous experiments that we proceed to describe.

We know that a pencil of common light, traversing a series of parallel plates of glass, or other diaphanous substance, at a certain inclination, becomes polarized perpendicularly to the plane of refraction; so that if a second series of plates be presented to the emergent rays at the same inclination, the light either passes through, or is in great part intercepted, accordingly as the second plane of refraction is disposed in a direction parallel or perpendicular to the first.

In order to see whether analogous effects are produced relatively to radiant heat, we have only to submit these two oblique piles of glass to trial by the thermomultiplier, disposing the planes of refraction successively in a parallel or in a perpendicular direction. But if the plates be sufficiently numerous, the quantity of emergent heat is very small, and scarcely appreciable by the most delicate instruments, especially for sources at a low temperature, the rays of which undergo an almost complete absorption in penetrating the first vitreous layers. Rock salt may be substituted for glass with the greatest success; but it is difficult to procure several plates of that substance sufficiently large and pure. To obviate, in a great degree, these various inconveniences, Mr. Forbes suggested the employment, for the polarization of heat, of mica reduced into very thin laminæ,

which then, like all other liquid and solid bodies in a very attenuated state, transmits notable portions of radiant heat emitted from any source whatever.*

We have noticed some of the results at which Mr. Forbes arrived by the employment of piles of mica. The calorific polarization obtained by means of two piles of ten plates each was far from complete, for it was always less than half, while for light it appeared about $\frac{9}{10}$. But the circumstance particularly worthy the attention of physicists, is the great difference which he observed in the proportion of heat polarized, according to the nature of the source: for in similar circumstances the same mica piles gave $\frac{29}{100}$ of polarization for the heat of an Argand lamp, $\frac{24}{100}$ for a Locatelli lamp, $\frac{36}{100}$ for a flame of alcohol, $\frac{40}{100}$ for incandescent platinum, $\frac{22}{100}$ for copper at a temperature of 390 to 400°, $\frac{17}{100}$ for an iron crucible heated by mercury at 280°, and $\frac{6}{100}$ for a vessel filled with boiling water.

These numbers, compared with the indices of polarization, which vary so greatly with the nature of the calorific rays given by the same system of tourmalines, might at first sight induce an opinion that the different species of heat are more or less susceptible of polarization. But by examining attentively the manner in which Mr. Forbes obtained his results, it may easily be seen that the numbers which have just been stated do not in the least represent the quantities of heat polarized. In fact, to measure and compare together these quantities of heat, Mr. Forbes has had recourse to the same method which I employed for the purpose of putting beyond doubt the constancy or variability of the calorific transmission of various diathermanous substances, in passing from one calorific source to the other,

* This fact results from a great number of experiments which I have performed upon glass, rock crystal, sulphate of lime, mica, water, alcohol, &c. It is intimately connected with the phænomenon of decreasing transmission presented in general by a given lamina successively exposed to radiant heat from sources at less and less elevated temperatures. It is also in strict relation with the greatly varied proportion of heat which passes through the same body when submitted to emergent calorific rays from different substances. In investigating the analogous effect in relation to light, we find, as I have shown elsewhere, (vol. lv. p. 361 of these Annals), [Scient. Mem. vol. i. p. 53], that all diaphanous substances, excepting rock salt, produce precisely analogous effects upon radiant heat to those of coloured media upon light; and in fact, the coloration which diminishes the transparency of bodies exposed to luminous rays of several qualities is entirely removed by reducing them into very thin laminæ, so that these tenuous layers then become equally permeable to all kinds of coloured rays.

viz. he varied the distance between the source and the thermoscope, in order to render nearly constant the quantity of heat radiated upon the instrument. Now it may easily be seen that the variation in the distance of the calorific focus cannot have any injurious influence upon the measures of transmission, because the diathermanous body placed against the opening of the intermediate screen being of very small dimensions, and the sources of heat being always at considerable distances, the most eccentric rays are never deflected more than a few degrees from the perpendicular, which produces no sensible alteration in the quantities of heat reflected and absorbed by the body submitted to radiation, as may be proved by direct experiment, by placing the same calorific source at different distances, and taking each time the transmission of a given lamina, which will be found constant, if everything be well arranged for observations of this nature. But the result is different in experiments of polarization by means of piles; for the proportion of polarized heat varies generally with the slightest variation of incidence of the calorific rays; and in the experiments under examination, the alteration in the inclination of these rays upon the piles would necessarily amount to several degrees, considering the proximity of the source to the thermoscope, the extent of the polarizing surfaces, and the absence of any intermediate diaphragm.

Besides, Mr. Forbes neglected to secure his thermomultiplier from the influence of the heat absorbed by the mica laminæ*,

* A single series of observations will suffice to show the small distance at which Mr. Forbes placed his sources of heat; and the very appreciable influence of the heating of the piles upon the results.

Source of heat, copper heated to 400° by an alcohol flame. *Distance of the thermoscopic body*, five inches and a half.

		Deviations of the galvanometer.
The plane of refraction of one pile at 0°, the other at	0°	$6\frac{1}{2}$°
,,	90	$5\frac{1}{4}$
,,	180	7
,,	270	6
,,	0	$7\frac{1}{4}$

(Trans. of the R. S. of Edin. vol. xiii. part i. p. 150.) London and Edinburgh Philosophical Magazine, vol. vi. p. 212.

The two piles were placed at the same inclination, in the interior of two graduated tubes, turning one within the other; the first was fixed upon the cylindrical envelope of the thermomultiplier, the second was free, and could move so as to direct the zero of the division successively into the positions indicated by the table. If the galvanometrical deviations corresponding to each of these positions represented the effect of the radiation transmitted immediately through the piles only, the values of the first, third, and fifth observations would be evidently equal to each other, and the case would be similar with the second

so that the effects observed represent the sum of the actions exerted upon the thermoscopic instrument by the two portions of heat which always co-exist in the phænomena dependent upon the passage of calorific rays by diathermanous substances, viz. the immediate transmission and the conductivity. The latter, in altering the absolute value of the index of polarization, would have allowed the equality in the proportion of heat polarized by sources of every description to subsist, could it have operated in these different cases with the same intensity; for all the calorific rays being equally polarizable, it is evident that the continuance of the action of heating cannot disturb the continuance of the effect due to the polarization. But the diathermancy of mica being analogous to that of glass, the quantity of heat which it absorbs, and consequently its proper heating, varies with the temperature of the source, and thus alters, by this variation of the perturbing cause, the constant effect produced by the principal cause.

In a new series of experiments which has appeared in the last volume of the Edinburgh *Philosophical Transactions*, Mr. Forbes has partly avoided the different incidence of the calorific rays upon the piles by placing the sources at a distance, always the same, but about three times greater than that which he had adopted in his previous researches. The results obtained have a nearer approach to equality. In fact, the index of polarization of the same system of piles inclined about 34° upon the axis of radiation was $\frac{72}{100}$ to $\frac{74}{100}$ for the Argand lamp, $\frac{72}{100}$ for incandescent platinum, $\frac{63}{100}$ for copper heated to 400°, $\frac{48}{100}$ for the iron crucible filled with mercury at 280°, and $\frac{44}{100}$ for the vessel full of boiling water*.

But the perturbation due to the heating of the piles still remained, and the existence of this cause of error, which Mr. Forbes allowed in the arrangement of his apparatus, is alone amply sufficient to account for the differences observed without its being necessary to suppose that different species of heat undergo, in a parity of circumstances, degrees of polarization so various. It might even be demonstrated that the influence of

and fourth observation. But instead of the two equalities, we find *increasing* quantities, which prove with the clearest evidence the progressive action of the heating of the piles.

* Trans. of the R. S. of Edin., vol. xiii. part ii., Researches on Heat, second series, p. 14. London and Edinburgh Philosophical Magazine, vol. xii. p. 551.

the heat acquired by the mica laminæ should operate in the direction indicated by Mr. Forbes' experiments; viz. that the action due to the heating of the piles should diminish the apparent index of polarization in proportion as the temperature of the source whence the radiation emanates is lower, as the following will show.

The heated piles transmit their own heat to the thermoscope; and if this secondary radiation be sensible, it always alters, as has just been remarked, the effect due to the heat polarized. But does the alteration produced tend to augment the real index of calorific polarization, or does it render it less apparent? In order to ascertain, I took some paper well blackened upon each side, which, in this state, is absolutely athermanous, but which, as is known, absorbs a large quantity of heat, and also radiates it in abundance.

I substituted a rectangle of this paper at the pile nearest the source, and concentrated upon it a large quantity of heat by means of a lens of rock salt. The *virtual* plane of refraction of the blackened paper was parallel to the plane of refraction of the posterior pile. The heat absorbed by the paper, and afterwards radiated upon the pile, heated the leaves of mica, and they threw the heat acquired upon the thermomultiplier placed at a little distance; the needle of the galvanometer gradually removed from zero in proportion as the mica became heated; but as the source was at a constant temperature, after five or six minutes, the quantity of heat acquired by the pile became equal to that lost by radiation and contact with the air, and the needle then had a permanent deviation, which in the circumstances under which I experimented was from 25 to 26°*. This being

* We know that spiders' threads do not burn when exposed to the solar rays concentrated by the action of the strongest lenses. From this isolated fact, some physicists have inferred that the heat acquired by thin plates, (*corps minces*) under the action of a constant calorific radiation, is in the inverse ratio of their thickness, and that it becomes null or insensible when they are of an extreme tenuity. This proposition cannot be true in all its generality, and is even completely false in several circumstances; for in the experiment described above, the effect of the heat of the blackened paper upon the thermomultiplier, instead of diminishing, constantly increased in proportion as the paper employed was thinner. It is needless to say that I had previously ascertained that this increase was not produced by an immediate transmission; which had no appreciable value in any of the sheets under experiment. Thus, in cases of this sort, the fact is directly contrary to the opinion I have alluded to; paper, and athermanous substances in general, when exposed to a constant source of heat, becoming more heated in proportion to their thinness: at least, when once they have attained their state of mobile calorific equilibrium, they radiate by their pos-

so, I turned the plane of refraction of the mica leaves perpendicularly to the virtual plane of refraction of the blackened paper, without on that account altering the common inclination of the laminæ upon the axis of calorific radiation: no difference was produced in the permanent deviation of the galvanometrical index, which, after a few minutes' oscillation, again stopped between 25° and 26°. The action due to the heating of the piles in experiments of polarization is therefore equal for the two directions, parallel and perpendicular, that are given to their plane of refraction*.

Now the index of polarization of a pair of piles at a given inclination, being only the difference between the two quantities immediately transmitted in the parallel and perpendicular positions of the planes of refraction, referred to the greater of them, it might be inferred that the action of the proper heating of the mica piles would *diminish* this index, by adding the same quantity to the two terms of relation. But mica becomes more

terior surface a quantity of heat which increases according to the diminution of their thickness. But is the fact the same with regard to diathermanous bodies?

If the impossibility of measuring the elevation of temperature of thin laminæ prevents the solution of this question by direct experiment, the properties actually known of immediate transmission furnish us with a satisfactory reply. In fact, glass, water, alum, and diaphanous substances the most refractory to the passage of calorific rays, admit the passage of notable quantities of heat thrown off by sources of all kinds, when reduced into thin laminæ; and as their faculties of transmission increase in proportion as the thickness diminishes, it is clear that in this case the quantity of heat retained will follow the contrary proportion, that is, the heating of the lamina will be in the direct ratio of its thickness. But this latter law requires invariability in the radiating source. It cannot always take place in cases in which the laminæ are submitted to rays of different origins, for these rays pass in various proportions by the same lamina, and, consequently, heat it in a degree proportionate to their intransmissibility. Certain sorts of heat that traverse in abundance a thin lamina, may therefore communicate to it a slight elevation of temperature, while others will heat it considerably by virtue of their feeble transmission through the substance of which it is composed; and if two laminæ, of the same substance, but of different thickness, be exposed to equal quantities of heat, thrown off by different sources, the thick lamina will become less heated than the thin one, if it receives the heat of the source whose rays are more transmissible.

According to all analogy, the substance which forms the spider's threads is extremely permeable to calorific radiation; on the other hand, heat emanating from the sun passes with greater facility through diathermanous bodies in general, than heat emitted from any other source. These two causes combined appear to be sufficient to account for the phænomenon of incombustibility presented by spiders' threads placed in the focus of lenses under the action of the solar rays.

* Mr. Forbes arrived at the same conclusion, by substituting for the anterior pile the sloping side of a metallic vessel containing hot water. London and Edinburgh Philosophical Magazine, March 1836, p. 218.

heated as the temperature of the source is reduced, since, like glass, it transmits immediately quantities of heat decreasing with this same temperature. If, therefore, the proper radiation of the mica piles exerts an appreciable action, the index of polarization will, in appearance, undergo *a greater diminution* for the sources at low temperatures than for those at elevated ones.

By the same principle of the secondary radiation of the piles another experiment of Mr. Forbes's may be explained, which, according to him, proves the *unequally polarizable nature* of calorific rays.

The radiant heat of copper raised to a temperature of 400°, by means of an alcohol lamp, gave him, as we have seen above, $\frac{63}{100}$ of polarization for the action of a certain system of mica piles. By interposing a lamina of glass between the same source and the same system of piles, the proportion of heat polarized increased ten hundredths, that is to say, when the heat traversed the glass lamina before being submitted to the polarizing action of the piles, seventy-three rays in a hundred, instead of $\frac{63}{100}$, disappeared in consequence of the intersection of the planes of refraction. The heat of incandescent platina having given him $\frac{72}{100}$ of polarization, without the interposition of the lamina, Mr. Forbes concludes from it that "the heat from a dark source, after transmission through glass, became as polarizable as that from incandescent platinum*." But it is easy to see that the lamina of glass interposed between the source and the piles of mica itself absorbed the greater part of the rays which previously heated these piles in the experiment of direct heat; so that the perturbing cause being considerably *enfeebled*, the apparent effect of polarization was *increased* to the point of becoming sensibly equal to that given by the rays of incandescent platinum, the passage of which through the mica excites in it a very slight elevation of temperature, on account of their great transmissibility through that substance.

The experiment shows that the radiant heat of incandescent platinum, and that of flame, traverses the thin mica leaves in nearly equal proportions†. This equality of heat transmitted being accompanied by an equality of heat absorbed, the piles must necessarily exert the same perturbation upon the imme-

* Researches on Heat, second series, by J. D. Forbes, p. 14. London and Edinburgh Philosophical Magazine, vol. xii. p. 551.

† Annales de Chimie et de Physique, vol. lv. p. 346.

diately transmitted rays of each source, which is the reason that Mr. Forbes found, in both cases, the same proportion of polarized heat.

Thus the action derived from the proper heating of the piles, an action varied by the nature of the source, or the interposition of a glass lamina, is of itself sufficient for the explanation of all the alterations observed by Mr. Forbes in the index of calorific polarization; and it has been already observed that the greater or less obliquity with which the various rays fall upon the polarizing laminæ may also produce analogous variations.

In order to obtain exact and comparable results, it is therefore necessary to avoid these two causes of error; for which purpose I have successfully employed the means which we proceed to examine. But it must first be seen how the piles of mica intended for experiments of polarization are prepared.

There are several different methods, but the following appears to be preferable: First, carefully determine, by any one of the known optical methods, the directions of the *axes* or *neutral sections* of luminous polarization for a natural sheet of mica, one or two millimeters in thickness, and cut, according to these two perpendicular directions, a rectangle eight or ten centimeters long, by four or five in width. Then take another rectangle of very thin card, a little larger than the piece of mica which has been cut, and remove all the inner part in a direction parallel to the sides, so as to form a rectangular frame, of which the opening will be six or eight millimeters smaller each way than the mica; then separate from the rectangle of mica, by means of a lancet, the thinnest lamina possible; fix it with a little gum upon the frame of card, carefully keeping its sides exactly parallel to those of the opening; and after having fastened upon those portions of the longer sides that rest upon the frame two narrow bands of gummed paper, detach a second lamina of mica, and superpose it exactly upon the first; cover the sides in the same manner with gummed paper, and proceed thus with all the laminæ subsequently separated from the rectangle of mica. When the pile is finished, place a second frame of thin card equal to the first on the top of it, apply some glue between the free parts of the two cards, and fasten together their exterior edges by bands of glued paper, in such a manner that no movement may take place in the sheets of mica, and that their sides may remain perfectly parallel or perpendicular to the sides of

the frame and to the neutral sections, one of which should always be found in the plane of refraction of the radiation, an indispensable condition, as is known, to render the polarizing action of these sorts of piles independent of their crystalline state, and consequently, similar to the action of piles of glass or any other amorphous substance. I procured thus four pairs of mica piles, composed of three, five, ten, and twenty laminæ.

The next step was to give them the necessary arrangement for experiments of polarization. The apparatus which appeared to me most suitable is exactly similar to that described in M. Biot's *Traité de Physique*, vol. iv. p. 255, with the exception of a few slight modifications, which render it still more simple, and more especially adapted to experiments of polarization by refraction.

It consists of a horizontal tube, to the two ends of which are adjusted two drums, open at one end, which by hard friction may be turned round, like the ordinary covers of cylindrical boxes; each of them is divided at the edge of friction into 360 degrees; from two opposite points of their free circumference proceed two arms parallel to the axis; they are pierced at a certain distance with two small holes, in which are fastened the pivots of a rectangular frame intended to receive the two piles of mica: these pivots, placed in a contrary direction upon the transverse line which passes through the centre of the frame, allow of its being more or less inclined in relation to the axis of the tube; they may be fixed in a determinate position by a clamp. The measure of the angle is furnished by an arc of the circle fixed upon one of the two salient arms of each drum.

Thus, when the piles are attached to the apparatus, they may be directly placed, by means of their moveable supports, at any inclination whatever in relation to the axis of the tube, and by afterwards turning the drums any possible position around this axis may be given them; that is to say, they may be made to travel over, in succession, all the imaginable angular positions around the calorific pencil; for we shall presently see that the rays of heat always enter the tube in the direction of the axis.

The circular divisions of the two drums mutually correspond by means of a line, parallel to the axis, traced at the upper part of the tube, and prolonged to the graduated edges in form of an index. The exterior arms being placed symmetrically upon the two sides of the tube, the reciprocal directions of the planes of refraction

of each pile may afterwards be known by means of the degrees marked by the two extremities of this line. Thus, when the drums both mark 0°, or 360°, these planes are parallel, and always preserve the same direction, if any number of degrees whatever upon each division be passed before the index. But if one drum be left at 0°, and all the degrees of its circumference be successively marked at the other, the plane of refraction of the second pile inclines more and more upon that of the first, becomes perpendicular at 90°, still advances towards the primitive direction, and reaches it at 180°. The same successive changes of inclination are afterwards produced further; that is to say, the planes of refraction are gradually separated, and take, at 270°, a perpendicular direction, in order to approach and resume the initial position of 0°, or 360°.

To avoid the diverse incidence of the rays upon the piles, I placed the source of heat in the focus of a lens of rock salt, sufficiently distant from the tube, and in the prolongation of its axis. A horizontal pencil of concentrated heat is thus obtained, which traverses the piles of mica in a direction parallel to the axis, and is propagated beyond, still preserving its cylindrical form, and a considerable portion of its primitive energy, which allows of the removal of the thermoscopic instrument intended to investigate the properties of calorific radiation in the different positions of the piles, to such a distance that the effect of their proper heating becomes perfectly insensible.

The employment of the salt lens has, therefore, two great advantages: 1. The giving of intense and sensibly parallel rays; 2. The possibility of completely securing the thermoscope from the influence of the heat absorbed by the mica laminæ.

As to the heating of the apparatus which supports the piles, that may be easily avoided by covering every part of it with a double or triple metallic screen, having an opening of an equal or smaller diameter than the smallest dimension of the laminæ.

We will now recapitulate, fixing our ideas by a particular example. Let the source of heat be a Locatelli lamp: the luminous and calorific rays emanating from it are received at a proper focal distance upon a lens of rock salt; they issue from it sensibly parallel and horizontal, travel over a free space of forty or fifty centimeters, reach the metallic screen which covers the polarizing apparatus, enter by its central opening, fall only upon

the piles, and traverse in a greater or smaller proportion the sheets of mica. We will suppose, for the sake of perspicuity, that each pile is composed of five laminæ, and that the planes of these laminæ are all parallel, vertical, and inclined 45° upon the axis of radiation. After emergence, the pencil of heat passes over another interval of from twenty to thirty centimeters, penetrates the envelope of the thermomultiplier, and, lastly, arrives at the anterior surface of the thermoscopic pile which transmits the impression received to the galvanometer. The indicating needle of the instrument commences its movement, and describes a certain angle, say of 35°·92.

Before proceeding to experiments of polarization, we must ascertain, 1. That the heat absorbed by the sheets of mica has no sensible influence upon the thermoscope; 2. That the effect observed is independent of the vertical direction of the planes of the two piles during their parallelism.

We may easily satisfy ourselves that these conditions are really fulfilled in the circumstances of the experiment, by first removing the thermoscopic body out of the space occupied by the pencil of transmitted heat, without increasing its distance from the last laminæ of mica, and still keeping the opening of its envelope directed towards them; and then reinstating the thermoscope in the direction of the calorific pencil, and turning, by means of the drums, the two piles of mica quite round the axis of the tube, without altering either their parallelism or their inclination. In effect, in the first case the needle of the galvanometer returns exactly to the zero of the dial*; and in the second, it gives constantly 35°·92 of deviation. The heating of the mica laminæ and the assumed vertical direction of their parallel planes have therefore no influence upon the results; and the deviation observed in any case of parallelism is produced

* In the supposed arrangement of the apparatus, the planes of the mica laminæ are vertical: the axis of the thermoscopic pile, which at first formed an angle of 45° with these planes, may, therefore, become perpendicular to them during its lateral movement. The thermoscope then receives anteriorly this same action, caused by the heating of the laminæ, which was previously exerted obliquely, and yet the needle of the galvanometer always returns to 0°. Therefore, the proper radiation of the piles of mica does not produce any appreciable effect.

It is evident that this demonstration should be repeated each time that the source of heat is changed, or its position relatively to the piles and the thermoscope altered.

solely by the heat directly transmitted by the piles inclined 45° upon the axis of radiation, whatever, in other respects, may be the particular position which they affect around it.

Now leave one of the drums at 0°, and place the other at 90°, or 270°: the common inclination of the piles upon the axis undergoes no alteration, but the planes of refraction deviate from their parallelism, and take a perpendicular direction; so that one of them, for example, being horizontal, the other necessarily becomes vertical. Now, upon transmitting the invariable radiation of the lamp through our ten laminæ thus disposed, we shall no longer have, as before, 35°·92 of deviation, but only 28°·54. There is then a very distinct diminution in the quantity of heat that reaches the thermoscope. *According to the two preliminary experiments just described*, this diminution can only be attributed to an effect of polarization.

The arcs of 35°·92, and 29°·54*, described by virtue of the primitive impulsions of the galvanometrical index, correspond to forces of 32·10; and 24·95. Dividing the difference of these two quantities by 32·10, and multiplying the quotient by 100, we have 22·06, a number which evidently represents the quantity of heat polarized by the pair of five laminæ, expressed in hundredths of the quantity transmitted when the two planes of refraction are parallel.

But this result was obtained at an incidence of 45°. In what direction is the variation of the polarizing action of the laminæ, when the angle which they form with the calorific rays is diminished? Is the proportion of heat polarized notably increased with the number of the laminæ? What degree of polarization may be reached by the concurrence of these two elements?

I have made several series of experiments, in order to resolve these different questions. Their results are laid down in eight tables, which we proceed to notice, first endeavouring clearly to explain the circumstances under which they were made.

I combined successively my eight piles, singly, two and two, and three and three; I thus formed of them eight pairs, composed of 3, 5, 10, 15, 20, 25, 30, and 35 laminæ. Each pair was then raised upon the apparatus, and exposed to the calorific flux of the lamp, in the parallel and perpendicular directions of the planes of refraction, and at different inclinations.

[* So in the original.]

The quantity of heat which reaches the thermoscope, at a given inclination of the piles, diminishes in proportion as the number of the laminæ increases. In order to operate as much as possible in the same circumstances, it appeared desirable to render the largest galvanometrical deviation produced in each of the eight series nearly constant. To effect this, I employed a small spherical metallic mirror, making the centre of curvature coincide with the middle of the flame; the concavity was turned towards the lens of rock salt. In this situation the calorific rays thrown off in the direction opposite the lens, were reflected upon themselves; and, being mixed with the heat thrown directly upon the lens by the flame, increased the intensity of the pencil parallel to the axis. I commenced each series by blackening the whole surface of the mirror by the flame of a resinous taper; then by partly removing the lamp-black with a linen cloth I gradually restored the metallic lustre upon a portion of its surface, increasing its extent until the intensity of the heat which reached the thermoscope at that inclination of the piles at which the maximum effect was obtained, had nearly attained the value of the largest galvanometrical deviation adopted, which was from 35° to 37°. It is almost superfluous to add, that I afterwards left the apparatus in the same state during the whole series of experiments having relation to the same pair of piles, so that all the quantities contained in each table may be compared together.

The titles inscribed at the head of each column sufficiently denote the objects to which the series of numbers which they contain relate. The first gives the angle under which the pair of piles meets the calorific pencil, which is measured from the surface. The second and fourth indicate the arcs, reckoned from 0°, described by the index of the galvanometer at the initial effect, when, in establishing the radiating communication with the source, the heat arrives upon the thermoscope, through the piles, in the two directions, parallel and perpendicular, of their planes of refraction: each of the numbers they contain has been established from a series of ten observations. The third and fifth columns contain the intensities of the forces corresponding to the arcs of the second and fourth. The last column comprehends the quantity of heat polarized in 100 transmitted rays when the planes of refraction are parallel; which quantity

is easily obtained, as has been seen above, by multiplying the difference of the two forces corresponding to the parallel and perpendicular positions by 100, and dividing the product by the first of those two numbers.

This polarized heat, or, in other terms, the heat which disappears in the act of the intersection of the two planes of refraction, is neither destroyed nor absorbed, but simply reflected, as occurs in the polarization of light. This may be proved by taking two of our bundles, composed of twenty or thirty laminæ, which are to be inclined from 30° to 40° upon the axis of radiation, and to receive at first a parallel and vertical direction. Afterwards withdraw the thermoscopic body from its place, and dispose it laterally at the same distance from the posterior pile, keeping it still turned towards it, but in such a manner that the axis of its cylindrical envelope forms, with the anterior sheet, an angle equal to that formed on the other side of the normal by the incident calorific pencil. The effect of the reflection, which should take place evidently in the direction of the thermoscopic body, is then extremely feeble, and the index of the galvanometer scarcely departs a few degrees from its natural position of equilibrium; for the heat transmitted by the first pile arrives upon the second, and continues to be transmitted by the remaining laminæ, in consequence of the parallelism of the planes of refraction. But if the anterior pile be turned in such a manner as to place its plane of refraction perpendicular to that of the posterior pile, leaving all the rest in their previous state, a considerable deviation is immediately manifested in the indicating needle of the galvanometer, which proves a very abundant reflection of heat upon the surface of the second pile: now, in experiments on polarization, it is precisely when the two planes of refraction are thus disposed, that a great portion of the heat no longer reaches the thermoscope.

The following are the eight tables.

TABLE I.

Inclination of the piles to the direction of the rays.		Calorific transmissions when the planes of refraction are				Quantity of heat polarized in a hundred transmitted rays, when the planes of refraction are parallel.
		PARALLEL.		PERPENDICULAR.		
		Arcs of Impulsion.	Forces.	Arcs of Impulsion.	Forces.	
Piles of three laminæ.	45°	35°·29	31·68	32°·01	29·12	8·08
	43	34·99	31·52	30·77	27·78	11·87
	41	34·24	21·12	29·55	26·18	15·87
	39	33·58	30·55	28·13	24·49	19·84
	37	32·84	29·81	26·22	22·70	23·85
	35	31·78	28·88	24·23	20·86	27·77
	33	30·71	27·70	21·98	18·87	31·87
	31	29·44	26·04	19·40	16·73	35·76
	29	27·41	23·81	16·53	14·35	39·73
	27	24·57	31·18	13·63	11·90	43·81
	25	21·24	18·25	10·94	9·54	47·73
	23	17·31	15·01	8·27	7·22	51·89
	21	13·31	11·63	5·88	5·15	55·72
	19	9·22	8·02	3·71	3·24	59·60
	17	5·02	4·39	1·83	1·60	63·55

TABLE II.

Piles of five laminæ.	45°	35°·92	32·01	28°·54	24·95	22·06
	43	35·69	31·89	27·01	23·45	26·46
	41	35·42	31·75	25·16	21·73	31·56
	39	35·21	31·64	23·47	20·15	36·31
	37	34·33	31·17	21·39	18·30	41·03
	35	33·30	30·26	19·73	16·46	45·61
	33	31·64	28·74	16·39	14·23	50·49
	31	29·71	26·38	13·80	12·03	54·39
	29	27·38	23·79	11·29	9·85	58·59
	27	23·70	20·36	8·72	7·61	62·62
	25	20·04	17·23	6·60	5·77	66·51
	23	16·01	13·91	4·74	4·14	70·24
	21	11·71	10·24	3·06	2·68	73·83
	19	7·58	6·63	1·71	1·50	77·37
	17	3·42	2·99	0·66	0·58	80·60

TABLE III.

Piles of ten laminæ.	45°	29°·82	26·53	17°·21	14·93	43·73
	43	31·41	28·49	16·48	14·31	49·77
	41	33·29	30·24	15·36	13·32	55·95
	39	35·19	31·63	13·95	16·16	61·56
	37	36·46	32·50	12·31	10·77	66·86
	35	36·86	32·88	10·63	9·26	71·84
	33	36·72	32·75	8·90	7·75	76·34
	31	33·79	30·76	6·92	6·05	80·33
	29	30·94	28·00	5·25	4·59	83·61
	27	27·89	24·25	3·72	3·25	86·60
	25	23·19	19·89	2·44	2·14	89·24
	23	17·60	15·26	1·55	1·36	91·09

TABLE IV.

	Inclination of the piles to the direction of the rays.	Calorific transmissions when the planes of refraction are PARALLEL. Arcs of Impulsion.	PARALLEL. Forces.	PERPENDICULAR. Arcs of Impulsion.	PERPENDICULAR. Forces.	Quantity of heat polarized in a hundred transmitted rays, when the planes of refraction are parallel.
Piles of fifteen laminæ.	45°	24°·12	20·75	9°·30	8·09	61·01
	43	27·08	23·51	8·95	7·79	66·87
	41	29·59	26·23	8·16	7·13	72·82
	39	31·66	28·76	7·23	6·32	78·03
	37	33·79	30·77	6·15	5·38	82·51
	35	35·58	31·83	4·99	4·36	86·30
	33	35·44	31·76	3·90	3·40	89·29
	31	32·13	29·22	2·90	2·54	91·31
	29	29·04	25·52	2·14	1·87	92·67
	27	24·41	21·03	1·55	1·36	93·53
	25	18·23	15·78	1·07	0·94	94·04
	23	12·05	10·54	0·68	0·60	94·31

TABLE V.

Piles of twenty laminæ.	45°	21°·23	18·24	6°·56	5·74	68·53
	43	24·60	21·23	6·51	5·69	73·20
	41	28·08	24·44	6·22	5·44	77·74
	39	30·66	27·63	5·68	4·97	82·01
	37	33·55	30·52	5·00	4·37	85·01
	35	36·21	32·25	4·24	3·70	88·53
	33	36·18	32·22	3·41	2·98	90·75
	31	34·60	29·50	2·52	2·21	92·51
	29	27·63	24·01	1·68	1·47	93·88
	27	21·52	18·49	1·13	0·99	94·64
	25	14·41	12·53	0·73	0·64	94·89
	23	8·31	7·26	0·41	0·36	95·04

TABLE VI.

Piles of twenty-five laminæ.	45°	18°·57	16·05	4°·17	3·64	77·32
	43	22·78	19·53	4·19	3·66	81·26
	41	26·51	22·97	4·00	3·49	84·81
	39	29·71	26·39	3·71	3·24	87·72
	37	32·45	29·48	3·28	2·84	90·33
	35	35·42	31·75	2·61	2·39	92·47
	33	35·56	31·82	2·20	1·93	93·93
	31	31·75	28·85	1·73	1·52	94·73
	29	27·20	23·62	1·33	1·17	95·05
	27	20·51	17·63	0·99	0·87	95·06
	25	13·13	11·48	0·65	0·57	95·03
	23	6·90	6·03	0·34	0·30	95·02

TABLE VII.

	Inclination of the piles to the direction of the rays.	Calorific transmissions when the planes of refraction are PARALLEL. Arcs of Impulsion.	PARALLEL. Forces.	PERPENDICULAR. Arcs of Impulsion.	PERPENDICULAR. Forces.	Quantity of heat polarized in a hundred transmitted rays, when the planes of refraction are parallel.
Piles of thirty laminæ.	45°	16°·92	14·68	2°·73	2·59	83·72
	43	21·50	18·47	2·74	2·40	87·01
	41	25·84	22·18	2·52	2·21	90·04
	39	29·36	25·93	2·30	2·01	92·25
	37	32·38	29·43	2·12	1·86	93·68
	35	35·96	32·03	1·90	1·67	94·79
	33	36·53	32·56	1·83	1·60	95·09
	31	31·90	29·01	1·62	1·42	95·11
	29	27·11	23·54	1·30	1·14	95·16
	27	19·89	17·13	0·94	0·83	95·15
	25	12·33	10·79	0·59	0·52	95·18
	23	8·81	8·08	0·28	0·25	95·08

TABLE VIII.

Piles of thirty-five laminæ.	45°	14°·69	12·75	1°·71	1·50	88·24
	43	19·35	16·69	1·72	1·51	90·95
	41	23·86	20·51	1·63	1·43	93·03
	39	27·99	24·34	1·56	1·37	94·35
	37	30·83	27·85	1·60	1·40	94·97
	35	33·88	30·86	1·74	1·52	95·07
	33	34·93	31·49	1·76	1·54	95·11
	31	30·89	27·93	1·57	1·38	95·06
	29	25·67	22·19	1·24	1·09	95·09
	27	18·23	15·78	0·88	0·77	95·12
	25	10·92	9·52	0·53	0·47	95·06
	23	4·34	3·79	0·22	0·19	94·99

From the various numerical results contained in these tables, we deduce the following consequences:

I. The proportion of heat polarized by the piles increases, as the angle at which the rays meet their surfaces is diminished.

II. In piles containing a sufficient number of elements, the calorific polarization attains a *maximum* effect, at a certain angle of inclination which it afterwards preserves for all the smaller inclinations that the ray may successively form with the laminæ.

III. The inclination which is always reckoned from the surface, at which the manifestation of the invariable effect commences, increases with the number of laminæ of which the piles are composed.

As to the value of this limit of polarization, it is nearly con-

stant in all the series, and is not much less than complete polarization, or $\frac{100}{100}$. It would, without doubt, arrive at it if the optical axes of the different mica laminæ which compose each of the two piles were exactly in the direction required for rendering the proper action of the crystallization totally inappreciable, and if the rays introduced into the system were all exactly parallel, conditions which it is extremely difficult, not to say impossible, rigorously to fulfil. By substituting my eye in the place of the thermoscopic body, when the two planes of refraction intersected, I constantly perceived, through the system, traces more or less decided of coloration. These colours, due to the action of the central laminæ, showed definitively that the light itself did not undergo a complete polarization under the influence of the mica piles: and I have little doubt that, had it been possible to measure with precision their degree of luminous polarization in the oblique positions, at which they gave their *maximum* effect, the value would have been found to be nearly $\frac{95}{100}$, as for the greatest effect of calorific polarization.

M. Biot had previously remarked that the proportion of light polarized by refraction, is increased indefinitely with the angle of incidence, so that the *maximum* effect occurs at the greatest degree of obliquity at which the rays of light can penetrate the substance of which the refracting laminæ are formed.

Sir D. Brewster has, besides, enunciated that the light of a wax candle, at a distance of from ten to twelve feet, is completely polarized, at an inclination of 10° 49′, by eight plates of crown glass; at 32° 50′, by twenty-seven plates; and at 48° 19′, by forty-seven plates; so that setting out from perpendicular incidence, the *angle limit*, at which complete polarization commences, is so much nearer the normal in proportion to the amplitude of the number of the polarizing laminæ.

The laws, therefore, of polarization by refraction, in reference both to heat and light, are exactly similar.

A very simple observation upon the numbers contained in the second or third column of the last six tables, will show that the calorific rays are also polarized by reflection; that in this case there is a given incidence at which the polarization takes place in the highest degree; and that the planes of the two polarizations, produced upon the radiant heat by the action of the forces of refraction and reflection, are respectively perpendicular.

If we look at an object through a lamina of glass, or any other

diaphanous substance, in a direction more and more inclined upon the plane of the lamina, it will be found gradually to diminish in intensity, in proportion to the increasing obliquity. It may readily be conceived that such would be the case; because the rays that fall obliquely upon the lamina traverse a greater thickness of glass than those which arrive in a direction more nearly approaching the perpendicular, and suffer consequently a greater absorption. But even if the matter of which the lamina is composed were perfectly limpid, and admitted the passage of all the light which penetrates into the interior, at any incidence whatever, the decrease of intensity corresponding to the increased inclination would still be observable, because the luminous rays undergo a partial reflection at the two surfaces of the lamina, which is at first feeble and sensibly constant, for angles of from 30° 40° around the normal, but which is rapidly augmented at increased inclinations, so that the pencil transmitted in a very oblique direction to the surfaces of the lamina, loses a very great portion of its intensity, solely on account of the reflection.

The same results are produced with two or several successive laminæ; but when the number is increased to about thirty, and beyond that number, the effects produced are very different.

If, for instance, a pile composed of forty or fifty plates of glass superposed, be held, first perpendicularly to the incident rays, and afterwards gradually inclined upon them, the feeble light transmitted under the normal incidence, instead of being diminished by an increase of obliquity, becomes, on the contrary, more and more vivid and brilliant, up to a certain inclination; it then loses by degrees the intensity acquired, and, lastly, becomes extinguished, when the rays by an excess of obliquity can no longer penetrate into the vitreous matter. Now, the angle at which the pencil transmitted attains its *maximum* intensity, is precisely that at which light is completely polarized by reflection. This singular deviation from the ordinary laws of transmission, is attributable, therefore, to a phænomenon of polarization. Suppose, first, for example, the pile inclined 35°·25′, the value of the angle at which light is completely polarized by reflection upon glass: the refracted rays at this inclination will be strongly polarized at a certain depth of the pile; for we have seen that light, as well as heat, is susceptible of complete polarization by refraction, at any angle whatever, if the laminæ traversed be sufficiently numerous. We also know that the plane of polariza-

tion of refracted light is perpendicular to the plane of refraction, or of reflection, at which reflected light is polarized. On the other hand, the rays polarized perpendicularly, to the plane of reflection, are no longer capable of being reflected from the laminæ of glass at 35° 25′, but penetrate into its substance without undergoing any diminution of intensity. Therefore, the refracted light in the interior of the pile, being completely polarized in a plane perpendicular to that of the refraction, after traversing a certain number of laminæ, and arriving upon the surfaces of the succeeding laminæ at an angle of 35° 25′, will experience the same negative effect; viz. it will traverse them all without suffering any loss by reflection. But this entire transmission cannot be effectuated at any other inclination, because, in that case, the luminous rays which are penetrated at a certain depth of the pile, and which become polarized by refraction, then undergo only an effect of incomplete polarization, by the action of the reflecting surfaces of the remaining laminæ, which consequently resume a portion of their ordinary activity, which increases in proportion to its further removal, in either direction, from 35°·25′. The losses of the luminous pencil will therefore follow the same progression, so that the *maximum* intensity, in transmitted light, will necessarily occur at the angle of complete polarization.

Thus the known fact of luminous polarization by reflection and refraction, and the equally known fact of the perpendicularity of the planes of these two polarizations, necessarily conduct to the consequence, that light transmitted by a pile of numerous diaphanous laminæ attains a *maximum* intensity at the angle of complete polarization, produced by reflection.

Vice versâ, starting from the observation of this *maximum*, in the quantity of light transmitted at different inclinations of the pile, we deduce from it the existence of the two polarizations, the angle at which polarization by reflection takes place completely, and the perpendicularity of the two planes at which the light is found polarized by virtue of the forces of reflection and refraction.

Now, this is precisely the case with the transmission of radiant caloric by piles of mica; for, by examining the series of numbers contained in the first columns of the last six tables, it will be seen that the transmission by the series of parallel laminæ increases with the inclination, up to an angle comprised between 33° and 35°, and decreases again beyond that limit.

The precise value of the angle at which the complete polarization of heat is effected, is not so easily obtainable as at first sight it appears to be. Indeed all the calorific rays do not traverse the mica laminæ in the form of the phænomena of polarization that we have just described; this may be proved by placing the laminæ perpendicularly to the incident pencil, for with this arrangement a sensible effect of heat is still obtained through the system. Now, we know that the polarizing action is null under the perpendicular incidence; there is, therefore, a portion of heat which passes independently of polarization; and though it cannot well be demonstrated excepting when the rays fall perpendicularly upon the laminæ, yet it is not the less certain that it must exist under every other incidence. If this portion of heat, transmitted independently of polarization, had the same value, whatever were the obliquity of the rays upon the laminæ, the angle under which the greatest calorific effect would occur would be always that of complete polarization by reflection. But this value varies with the incidence, according to a progression that differs entirely from the law observed by that portion which passes by virtue of the polarizing forces; for we have seen that instead of first increasing, as this does, until it reach the angle of complete polarization, it constantly decreases from 90° of incidence to 0°. If the influence of non-polarized heat, upon the transmission of polarized heat, be sensible, it must produce a displacement in the position of the *maximum*, and bring it evidently nearer the perpendicular incidence.

To obtain security from this cause of error, it may be observed, that the absolute quantity of non-polarized heat which traverses the laminæ diminishes as their number increases. The probable error, in the determination of the angle of polarization, will therefore follow the same decreasing progression, and become null for a series composed of a sufficient number of laminæ.

In accordance with this principle, I procured a supplementary pile of forty-four elements, which, added to the other piles, formed a series of a hundred and twenty laminæ. Here the quantity of non-polarized heat could have no appreciable influence upon the calorific rays that traverse the system by virtue of the polarizing forces; for the transmission was sensibly null under the perpendicular incidence. Thus, the maximum transmission, in oblique incidences, would give exactly the angle of complete polarization.

I therefore mounted my hundred and twenty laminæ on one frame, which was provided with two pivots upon the transversal section, and an alhidade indicating the inclination of the polarizing surfaces to the calorific rays, upon a vertical circle ten inches in diameter. The transmissions observed at each half degree comprised between 33° and 35°, are subjoined: and to them are added two series of similar observations made upon two piles consisting of a smaller number of laminæ, one of twenty, the other of sixty elements. The quantity of incident heat varies from one series to another, and consequently the transmissions given under the same inclinations by the three systems of laminæ, cannot be compared together. The fulfilment of this condition of comparison was neglected, in order to render the transmissions from the series consisting of numerous laminæ more sensible; nor was it of utility for the end actually proposed.

Inclination of the Laminæ to the Rays.	Calorific transmissions by		
	20 Laminæ.	60 Laminæ.	120 Laminæ.
35°·00	37°·34	35°·97	31°·86
34·30	37·42	36·48	32·71
34·00	37·46	36·87	33·07
33·30	37·39	37·10	33·29
33·00	37·09	36·82	33·02

It will be perceived from a glance at this table, that the greatest calorific transmission occurs at an incidence of 34° in the first series, at 33° 30′ in the second, and that it maintains the same obliquity in the third. The influence of the non-polarized heat upon the value of the angle of polarization, is therefore sensible only when the pile is composed of a small number of laminæ. By comparing, in each of the last two series, the number which represents the greatest transmission, with the numbers that immediately precede and follow it, it will easily be seen that the *maximum* cannot differ much from 33° 30′, and that if there be a deviation of a few seconds, it is rather in the direction of the 34th degree than in the opposite one.

According to the law discovered by Sir D. Brewster, the tangent of the angle of polarization, for light, is given by the number which represents the index of refraction of the body employed as a reflector. Now, we know that mica has an index of refrac-

tion equal to 1·5*; the angle corresponding to this quantity, taken as a tangent, is 56° 19′, or 33° 41′ reckoning from the surface.

Thus, the angle of complete polarization, by reflection, is very nearly the same for both heat and light†.

Now take a pair of piles, each of twenty laminæ, and after mounting them properly upon the apparatus of polarization, place a lamina of alum, amber, or black glass, or a layer of water, oil, or some other diathermanous substance, against the opening of the screen which covers the apparatus. The emergent rays of the layer added to the system, will then pass through the two packets of mica, which are to receive in succession the two directions adapted for measuring the quantity of heat polarized by refraction. Now, in effectuating this experiment it will be found that the index of polarization does not alter in the smallest degree with the nature of the substance interposed, and that its value coincides precisely with the proportion of heat polarized under the actual incidence of the two piles, when the opening of the screen is free.

To exhibit this fact with facility, and in a very evident manner, I employ a means which to me appears capable of carrying conviction even to the most prejudiced mind. I choose two substances endowed with *contrary diathermancies*‡, that is to say, two bodies which, when exposed to the same calorific flux, admit

* Biot, *Traité de Physique*, vol. iv. p. 80.

† From what precedes, it will easily be conceived that to polarize heat or light by means of refraction, it is nearly always requisite to give the piles a great degree of obliquity upon the incident rays. When the laminæ are sufficiently numerous we may stop at the inclination at which complete polarization commences, which, in certain cases, allows of placing the piles at incidences nearly approaching the perpendicular. However, when the two series of plates consist of very many elements, it is often useful to dispose them, in preference, at the angle of complete polarization by reflection, in order to have an abundant transmission of luminous or calorific rays.

‡ Experiments have just been commenced at Geneva, upon the quantity of heat radiated by bodies under a serene sky, at different hours of the day. An account of them may be found in the number for April, 1837, of the *Bibliothèque Universelle*, in which one of the learned editors of that excellent repertory has discussed the results of those observations under the title ot *Diathermansie de l'Atmosphère*. The word diathermancy, as I have defined it in my second memoir upon Transmission, (vol. lv., p. 378 of these Annals) signifies the property possessed by nearly all diathermanous bodies, of admitting the passage only of certain species of calorific rays. When we wish to denote the *quantity* of heat transmitted independently of the *quality*, the term *diathermaneity* is perhaps preferable, in order to preserve the same termination as the word *diaphaneity*, indicating the analogous property in relation to light.

the passage of rays of so distinct a nature, that the emergent heat of the first can scarcely pass by the second, and *vice versa*; of them I form laminæ of such thicknesses that the quantities of heat transmitted through each of them and the whole of the two piles may be equal; I then place one of these laminæ against the opening of the screen, and observe the calorific actions produced by the rays which reach the thermoscopic body in the two principal directions of the planes of refraction of the mica piles. I repeat the same observations with another lamina, and obtain exactly the same deviations upon the galvanometer.

If we take in the table of reduction the forces corresponding to the two galvanometrical deflections observed in either case, and calculate the index of polarization from these data, we shall have a value equal to that indicated by Table V., viz. for example, $\frac{77}{100}$, $\frac{88 \cdot 5}{100}$, or $\frac{93 \cdot 9}{100}$, according as the calorific rays traverse the piles under the obliquities of 41°, 35°, or 29°; and that whatever be the nature of the lamina placed against the opening.

The substances best adapted for these experiments of comparison are opake black glass, or green glass which is impermeable to red rays, on the one hand; and water, citric acid, or alum, on the other. It may be remembered that the heat transmitted by this latter class of bodies undergoes, under the influence of tourmalines, a polarization which reaches $\frac{96}{100}$, while the emergent heat of the antagonistic substances, green or black glasses, submitted to the same polarizing system, give scarcely any sensible trace of this phænomenon, the apparent index of polarization being, in certain cases, scarcely elevated to one or two hundredths. And then these indices, determined by the system of the two piles, no longer present any appreciable difference. Thus the calorific fluxes transmitted by bodies of different natures, and which fluxes are of a constitution so different, are all equally polarizable by refraction; which proves that the polarization produced by the refractive forces of the media is independent of the quality of the calorific rays.

Though this consequence is rigorously established by the experiments that have been just related, it appeared to be not altogether useless to verify it also upon calorific rays emanating from different sources. To this end I replaced the Locatelli lamp by a spiral of platina maintained in a state of incandescence, by means of the flame of alcohol. The indices of polarization were again equal to those indicated by our eight tables. The same thing

happens when, instead of incandescent platina, a metallic lamina heated to 400° is employed, or simply a vessel filled with boiling water.

But the heat of these two latter sources being very slightly transmissible by the mica, and consequently unable to traverse piles composed of a great number of laminæ, notwithstanding the action of the salt lens by which their parallelism is established, I receive the *parallel rays* emerging from the apparatus of polarization upon a second lens of rock salt, which collects them all, and concentrates them upon the thermoscopic body. *The divergent rays*, which proceed from the heating of the posterior pile, must be weakened until they become perfectly insensible, either by removing the collecting lens to a suitable distance, or by bringing it very near. In the first case these rays are more and more dispersed by their natural divergence, and arrive upon the collector-lens with too little intensity to give an appreciable effect. In the second case, the central parts of the last sheets of mica being within the principal focal distance, the greater part of their proper rays of heat, instead of being concentrated and consequently mixed with the direct heat, are, on the contrary, dispersed by the lens still more rapidly than is effected by their natural divergence, and have no action whatever at a very short distance. Whatever be the means adopted, care should always be taken, after the collector is added, to ensure that the condition of the insensibility of the thermoscope to the heating of the piles is exactly fulfilled. For this purpose the anterior pile is to be removed from its frame, and in its place is to be substituted, as in the experiment at page 146, a sheet of paper blackened upon each side, which becomes as much heated as mica, and even more, because it does not immediately transmit radiant heat. If everything be well arranged, no appreciable calorific action is obtained. In the apparatus which I possess, the use of the collector about doubles the intensity of the effects, while preserving, according to the method just indicated, the direct rays completely pure, and without mixture with the secondary heat of the piles.

*Experiments of polarization may be thus carried, with the

* It is evident that more might be gained with piles of mica, and a lens of larger dimensions. To attain this amplification of the thermoscopic effects, Mr. Forbes, in his second series of experiments upon polarization, employed the conical reflector of brass that M. Gourjon generally uses, in addition to my apparatus of transmission; but this reflector collects at the same time the direct heat of the source, and that proceeding from the heating of the piles,

obscure heat of copper at 400°, to the limit of $\frac{95}{100}$, already obtained by means of the direct heat of flame. It is impossible to attain this limit with the heat of a vessel filled with boiling water, because the mica exerts upon it an action too strongly absorbent

as we have shown when considering the results of his observations. Mr. Forbes appears to attribute the application of the reflector to the thermo-electrical piles to M. Nobili. Another physicist, M. Despretz, says, in the last edition of his *Traité de Physique*, that the thermomultiplier which I employ is due entirely to M. Nobili, and that I have only rendered its indications regular. Perhaps I may be here allowed to state the real facts.

The first idea of measuring temperatures by thermo-electrical currents is due to M. Becquerel. His object being to estimate high degrees of heat, he constructed his *electrical thermometer* of wires of platina and palladium, which he put in communication with a multiplying galvanometer, made according to the principles of Poggendorf. A few years later M. Nobili proposed the employment of thermo-electricity, for the production of a *thermoscope of contact* superior in sensibility to that of the late M. Fourier, which consists of a common thermometer, around which is tied a small bag of flexible skin, filled with mercury. For this purpose he made use of bismuth and antimony, which develope the maximum thermo-electrical effect; of these substances he formed a pile, which he immersed almost entirely in a wooden cylindrical box, containing liquid mastic, so as to leave exposed only the superior alternate contacts, which were polished and reduced into the same plane; two bars of copper passing through the sides served to establish the communications with the two ends of an astatic galvanometer. The box was held in the hand, and the bodies, whose differences of temperature were sought, were touched with the uncovered face of the pile. The elements of this pile were twelve in number, (six pairs) folded over rectangularly, and in a contrary direction at the two extremities, in order to prevent the contact of the intermediate parts when they were soldered together. Their section was from forty to fifty square millimetres, and the diameter of the box from two to three inches. Upon the instrument thus constructed, I commenced my labours to convert it into a *thermoscope of radiation.* Having observed in some preliminary trials, that the action upon the multiplier depended much more upon the number than upon the bulk of the elements, and likewise, that the thermo-electrical currents never acquired, within certain limits, the tension necessary for traversing non-metallic bodies; I gave the elements the form of small flat bars, from thirty to forty times lighter than M. Nobili's, and kept them insulated in their whole length, except at the extremities where the solder was placed, by small bands of paper; I increased the number of them considerably, and fixed them by the middle upon an operculum adapted to a transversal ring seven or eight lines in diameter, and low enough for the two extremities and a great part of the rest to be perfectly free. I then covered all the salient parts of the pile with lamp-black, and surrounded them with cylindrical tubes, or conical reflectors, according as my object was to appreciate the action of a small pencil of parallel rays, or to collect the divergent heat proceeding from the walls of a room, or any other large distant surface. Lastly, I gave it the form and the proportions of the thermomultipliers so skilfully constructed by M. Gourjon, and which are now to be found in the principal collections of philosophical instruments, both at Paris and in foreign countries.

The advantage obtained by diminishing considerably the transversal section of the elements, is not only that of being able to introduce a larger number into a very small space, and thus to increase the tension of the electric current which is to traverse the long wire of the galvanometer, but it is specially useful in preventing the formation of the returning currents which took place in the

to allow it to traverse a considerable number of laminæ, while maintaining a sufficient intensity; but, happily, this experiment is not necessary to prove that calorific rays proceeding from different sources have an equal aptitude for polarization. It is sufficient to show that, under the action of a given number of laminæ, placed at a determined inclination, every species of heat, when rendered parallel by means of a rock-salt lens, and separated from the rays produced by the variable heating of the polarizing piles, gives indices of polarization sensibly equal. Piles composed of a small number of elements which furnish a sufficiently abundant transmission of heat from any source whatever, may be very advantageously employed for this purpose.

The indices of polarization are easily calculated from the data of observation, by means of the table which furnishes the ratios between the forces, and the deviations of the magnetic needles of the galvanometer; but if we would be independent of this table, and show by the simple inspection of the movements of the galvanometrical index, the equal polarization of radiant heat thrown off by sources of different temperatures, incandescent platina, and copper at 400°, for example, an artifice must be employed analogous to that recently described when speaking of the calorific rays transmitted by different species of bodies exposed to the radiation of flame.

After observing the greatest calorific effect obtainable by means of the heat derived from copper at 400°, we must again take incandescent platina, and interpose one or more plates of

interior of M. Nobili's pile, and destroyed a part of the effect produced. The mastic which covered one of the faces of his pile was also a great inconvenience, for it hindered the exterior thermometrical variations from communicating with equal rapidity with all the metallic parts, the consequence of which was, that deviations of from 30° to 40° were produced, during whole hours, solely by the difference of temperature between the mastic and the ambient air. At last, by substituting polished metal for wood in the construction of the envelopes, the instrument was secured from the exterior calorific radiations, and the observer thereby enabled to approach it, without the apprehension that the heat of his own body would injure his experiments.

The greater part of the alterations that I made in the thermo-electrical pile, are described in a note published by M. Nobili himself, who so far recognised their importance as to say, "In future I shall combine a *second pile* of this kind with *my first thermomultiplier*." (*Bibliothèque Universelle*, vol. xliv., p. 233.) But from that time the original *pile of contact* was really of little importance, which was the reason that M. Nobili thought it just and proper to add my name to his when the electrical thermometer actually in use, that is, the thermomultiplier *for the measurement of radiant heat*, was presented to the Institut, September 5th, 1831.

glass in the course of the rays transmitted by the piles with parallel planes of refraction, in order considerably to diminish the more intense energy of the calorific radiation, and render it equal to that from the feeble source, when the planes of refraction of the piles are also parallel. We afterwards dispose these planes perpendicularly, and the index of the galvanometer descends precisely the same quantity in both cases.

Sir D. Brewster found that to arrive at the limit of the obliquity at which the polarization of light becomes complete, by means of refraction, the number of laminæ requisite diminishes as their refractive power increases. The refrangibility of each coloured ray that enters into the composition of white light diminishes gradually from the violet to the red; therefore, for a certain series of laminæ disposed at a determinate inclination, inferior to the angle which is the limit of complete polarization, the quantity of light polarized will be greater for the violet rays than for the blue, greater for the blue than the green, &c.

Analogy induces us to believe that similar phænomena occur with respect to the different species of calorific rays which we have frequently compared to light of various colours. But, as we have just seen, these variations entirely escape the existing resources of calorimetry. Nor can this circumstance occasion much surprise if we consider, I. that in the case of light the differences between the quantities polarized by glass or mica, acting at a given incidence upon violet and red, which are the rays of the greatest and least refrangibility, do not much exceed the hundredth part of the entire quantity, even in the most favourable circumstances; II. that these small variations would probably not have been discovered and measured, if the criterion of coloration, which enables the eye immediately to distinguish luminous rays of different refrangibility, had been wanting in light as well as in heat; III. that the differences of refraction of the divers rays of heat proceeding from terrestrial sources are very small, and only exceed the amount of the analogous variations of light by a scarcely sensible quantity*; IV. that we can never operate alone upon one sort of calorific rays, since every direct flux of heat contains several species, which pass, in groups more or less complex, through the piles of mica and other laminæ interposed, and consequently give a species of interme-

* Vol. lv. p. 368, of the *Annales de Chim. et de Phys.* [or SCIENT. MEM. vol. i. p. 56. EDIT.]

diate index between the extremes, the values of which already approach so nearly.

The variable calorific transmission of a series of numerous parallel laminæ, presented under increasing inclinations to the radiation of flame, has recently led us to the inference that heat like light is polarized by reflection; that is to say, that this species of polarization takes place in a plane perpendicular to that of heat polarized by virtue of the refracting forces, and that the angle at which it is completely effectuated differs by a scarcely appreciable quantity from that given by the complete polarization of light. It may here be added that this angle does not undergo any sensible variation if the nature of the calorific radiation be altered, either by the interposition of laminæ of different diathermancy, or by substituting other sources of heat for flame. The emergent rays of opake black glass, transmitted by a pile of seventy laminæ, give actions upon my apparatus, which, at an inclination of 33° 30′, the moment of the maximum effect, push the index to more than 30°, and allow it to descend rapidly towards zero when the laminæ are inclined in either direction. The direct rays of copper heated to 400° produce the same relations of intensity at different inclinations, but upon a much smaller scale.

I shall here observe, once for all, that in the greater number of experiments on calorific polarization, in which rays of heat unmixed with light are required, the obscure heat of bodies below incandescence may be very advantageously replaced by the emergent heat of perfectly opake black glass exposed to the calorific fluxes of flame or incandescent platina. For this sort of heat is certainly perfectly obscure, and, in addition, is endowed with a diathermancy very analogous to that of mica; it consequently presents all the conditions desirable for the verification, upon heat alone, of the facts corresponding to those observable in luminous polarization.

The invariability manifested in the angle of the complete polarization of heat by reflection, notwithstanding the differences of the mean indices of the refraction of the various incident pencils, may be conceived relatively to the limits of precision furnished by our actual calorimetrical instruments, from reasons exactly analogous to those that have just been alleged when treating of polarization by refraction.

And even should we at some future time succeed in insulating

the different calorific rays, and in measuring their indices of polarization for a given incidence with the greatest exactitude, we shall only add a new element to the science of radiant heat, which will separate, by a few *fractions of a degree*, the inclinations actually known, at which the different species of rays give the same quantity of polarization. But all these species being susceptible of complete polarization, will still, notwithstanding these little distinctions, be of the same *polarizable nature*.

Heat then is polarized absolutely, like light, by refraction and reflection; a conclusion which fully confirms the theory developed in the first part of this memoir, to show how the phænomena of polarization may take place in the interior of tourmalines without the possibility of perceiving them outwardly*. Indeed, it may be remembered that certain species of tourmalines give a calorific polarization, which is either total, incomplete, or null in appearance, according to the quality of the heat employed. But we have just seen that all the calorific rays are equally polarizable: there exists, therefore, in the tourmalines, a cause which sometimes conceals, and sometimes exhibits the polarizing action. This cause can only be double refraction, which always produces two superposed pencils, equally intense, but differently polarized, in plates divided parallel to the axis of crystallization. When the action of the tourmalines manifests itself, one of these pencils is completely absorbed, and the other remains alone and exhibits its proper direction of polarization; in the opposite case, the two pencils undergo an equal absorption, and issue together completely neutralized with regard to polarization. Now if in the latter case the emergent heat resembles ordinary heat, the second pencil, which was previously absorbed, must necessarily be polarized at right angles to the other; its polarization must also be complete, for it is in that state that the first pencil of heat is separately exhibited.

The production of two calorific pencils in bi-refractive media, and their rectangular polarization, is also inferred from another experiment exactly analogous to those performed in optics to show the action which bodies possessing double refraction exert upon polarized light.

If a ray of light, reflected by a mirror of black glass, at an angle of 30° 25′, traverse perpendicularly a lamina of sulphate

* Vol. lxi. p. 408, of the *Annales de Chim. et de Phys.* [or SCIENT. MEM. vol. i. p. 315. EDIT.]

of lime or mica, and afterwards fall upon a second surface of glass at an equal inclination of 30° 25′, the latter reflects the incident light in a larger or smaller quantity, according to the positions which the principal section of the crystallized lamina and the plane of the second reflection affect, in relation to the plane of primitive reflection, in which the ray of light is first polarized.

Let us consider the two mirrors independently of the crystallized lamina. If we first make their planes of reflection to coincide, and afterwards place them perpendicularly, in the first position we shall obtain the maximum of reflected light, in the second the minimum. The effect is unaltered if the doubly refracting lamina be interposed between the two mirrors, after tracing upon its edges the direction of the principal section, and bringing it parallel or perpendicular to the primitive plane of reflection; the proportions of light reflected by the second mirror remain the same in each case; hence the denomination of *neutral axes*, given to these two directions of the lamina. But if the principal section or its perpendicular be inclined in such a manner that one of them forms an angle of 45° with the plane of primitive polarization, there is a very considerable alteration in the reflection of the second mirror; the maximum of reflected light is diminished, the minimum increased; and the diminution of intensity produced in the first case, when the planes of reflection are parallel, is found to be precisely equal to the augmentation which occurs in the second case, when the planes of reflection are perpendicular.

These variations of intensity, caused by the particular position of the principal section of the bi-refracting crystal, in relation to the plane of primitive polarization, require for their production a certain thickness according to its nature, but always extremely small, of the lamina interposed: they are besides accompanied by a brilliant coloration, which ceases also at certain limits of thickness, equally dependent upon the quality of the crystal interposed. We here lay aside the subject of colours, and shall only consider the intensity, which always follows the law enunciated whether the colours be perceived or not, the reflected light in the latter case appearing perfectly white, as happens with plates of sulphate of lime rather more than a demi-millimetre in thickness, and with plates of mica nearly twice as thick.

It would be superfluous, for the end proposed, to enter into all the theoretical details relative to the different modifications that the lamina interposed impresses upon the luminous pencil in proportion as its principal section turns around the plane of primitive polarization; they may be found in all optical treatises. Let it suffice that we call to mind that the equality of the two variations of which we have recently treated, is a necessary consequence of the double refraction and the complete and rectangular polarization that the luminous pencil undergoes in the interior of the bi-refracting crystal. The light polarized by the first mirror, when traversing this thin crystal, is either subdivided into two parts sensibly superposed, or preserves its unity, according as any one of the neutral axes is inclined, or parallel to the plane of primitive polarization. When the subdivision takes place, there results from it, at the particular inclination of 45°, two pencils of equal intensity, ordinary and extraordinary, which, in the two cases under examination, always have their planes of polarization so turned that one of them is found precisely comprised in the plane of reflection of the second mirror, and the other in the perpendicular direction: it is the first only which can undergo the second reflection and reach the eye. Now one of these two pencils is sometimes added to the light reflected by the second mirror, and sometimes subtracted, which is the reason that the augmentation produced, when the planes of reflection are perpendicular, is equal to the diminution which takes place when these planes are parallel.

The results just related do not absolutely require the employment of two mirrors, but may be also obtained with a pair of tourmalines, whose axes are rendered successively parallel or perpendicular. They may also be observed by means of two series of parallel laminæ of glass, properly inclined to the incident rays, and so disposed as that their planes of refraction are sometimes parallel, at others perpendicular.

Now, if the same phænomena can be produced upon calorific rays, we may conclude that heat is refracted and polarized, like light, in bodies possessed of double refraction. This experiment was tried by Mr. Forbes with two of his mica piles, giving from twenty to thirty hundredths of sensible polarization, between which he interposed a large vertical lamina of mica, which was provided with two contiguous bases, and inclined to each at an angle of 135°. The principal section having a direc-

tion perpendicular to one of the two sides of which this double basis was formed, and the plane of primitive refraction remaining always vertical, by resting the lamina first upon one, then upon the other basis, and disposing the second plane of refraction, first vertically, then horizontally, the actions indicated in the following table were obtained.

Sources of Heat.	Variation, in degrees of the thermomultiplier, observed when the principal section of the interposed lamina of mica passes from the vertical to 45° of obliquity, whilst the plane of refraction of the second pile is	
	Vertical.	Horizontal.
Mercury at 280°.........	− 0°·23	+ 0°·26
Copper at 400°	− 0°·517	+ 0°·545
Incandescent Platina...	− 2°·18	+ 2°·32
Argand Lamp............	− 1°·43	+ 1°·37

By comparing each positive variation with the corresponding negative variation, we see that the first is constantly superior to the second, for the two sources of heat possessing light, and incandescent platina; and the contrary for the radiation of the lamp; the difference, which is five or six hundredths for the three latter cases, amounts to nearly twelve-hundredths for the first. But, from the nature of the experiments, says Mr. Forbes, the table tends generally to show the coincidence of the two variations*. I know not whether this tendency will appear sufficient to physicists in general.

It is really alarming when we see that the effects produced scarcely amount to a few fractions of a degree for obscure heat, the subject in which we are chiefly interested; for it is very difficult to estimate smaller quantities than a quarter of a degree upon the circle of the thermomultiplier, which is scarcely more than five centimetres in diameter: on the other hand, circumstances, in appearance the most unimportant, may produce variations equal at least in extent to the deviations observed in the first two cases. It is true Mr. Forbes has adapted a micrometrical system to his galvanometer, by means of which he can, with greater ease, appreciate, as he thinks, even tenths of a degree; he also endeavours to secure himself from perturbing causes

* The following are the author's own expressions: "The table generally points to a coincidence, and that as close as by the nature of the experiments we should perhaps be warranted in expecting."—Trans. of the R. S. of E. vol. xiii. p. 162., [or Lond. and Edinb. Phil. Mag. vol. vi. p. 366. Edit.]

by taking the mean of several observations. But these expedients were not sufficient in the case under consideration, as is even evident from the nature of the results obtained upon the rays of obscure heat, which, though giving a tolerably considerable difference, and always in the same direction, would yet be far from proving the equality of the two actions, if it were not deducible from the analogous case of light, in which this equality is established from inductions that cannot admit of the least doubt.

To render the experiment conclusive of itself, it must be performed upon an obscure calorific flux, very intense and very transmissible through mica, in order to be able to polarize it almost completely by piles of numerous laminæ, while still preserving a notable portion of its energy, and to render it, thus strongly polarized, more sensible to the doubly refracting action of the interposed laminæ. It must also be secured from the heating of the mica system, which always tends to diminish the apparent effects of polarization. Nothing is more effectual for satisfying these conditions than our calorific rays rendered parallel by the rock-salt lens, and completely separated from light, and from the greater portion of the heat absorbable by the mica, by their previous transmission through opake black glass. I therefore caused a pencil of this obscure heat to fall upon my two piles of twenty laminæ inclined 33° 30′ upon the axis of radiation; I placed between them the perpendicular lamina of mica, and after ascertaining, by the means indicated above, that the proper heat radiated by the last pile upon the thermoscopic body was insensible, I proceeded to the measurement of the two variations, which were then very considerable, as may be seen from the following table.

Origin of the obscure rays transmitted by opake black glass.	Variation, in degrees of the thermomultiplier, observed when the neutral axes of the interposed lamina pass, relatively to the plane of refraction of the first pile, from parallel and perpendicular directions to an inclination of 45°, while the plane of refraction of the second pile in relation to it is	
	Parallel.	Perpendicular.
Argand Lamp............	− 29°·32	+ 29°·27
Locatelli Lamp	− 27°·51	+ 27°·56
Incandescent Platina...	− 31°·19	+ 31°·15

Each of the three sources of heat was placed at the centre of a spherical reflector; the calorific pencil of parallel rays, after having traversed the black glass and the system of mica laminæ,

arrived upon the thermoscopic body, without being there condensed by the collector, which was not in the least required, in consequence of the intensity of the effects produced. The mica lamina, interposed between the two piles, was of a circular form, and in thickness equal to $0^{mm} \cdot 2489$; it could only revolve in its own plane around the centre, which consequently remained immobile during this rotatory motion.

The equality of the negative, and the corresponding positive variations, is here established with all the requisite exactitude, for their differences are less than $\frac{1}{500}$, sometimes in one, and sometimes in the other direction. Yet each number contained in this table is the result of only ten observations. It is true that these observations were made with the greatest care, and that the differences between the maximum and minimum of each series scarcely exceeded half a degree.

Now, suppose that a horizontal pencil of obscure heat issuing from black glass be thrown upon a vertical surface of glass or mica, at the angle of complete polarization; that the reflected rays be afterwards transmitted perpendicularly through the circular lamina of mica; and that the emergent heat be received upon another surface of glass or mica, disposed parallel to the first; it will there undergo a second reflection, and return in a direction parallel to the primitive direction, but always removing further from the source. If the thermoscopic pile be placed at a certain distance from the two reflectors, so that it may receive the impression of the pencil of heat which has undergone the two reflections and the intermediate transmission of the mica disc, by turning this disc in its proper plane, a much less energetic action is observable when the principal section is inclined 45° upon the horizon, than when it is horizontal or vertical. The effects obtained are nearly as sensible as the differences recorded in the preceding table; for the index of the galvanometer, in passing from one position of the principal section to the other, travels over arcs of from 20° to 25°.

This experiment, which is perfectly analogous to the preceding ones, is very interesting, as it enables us completely to insulate, as to their mode of manifestation, the polarizing forces developed in the act of reflection, from the similar forces developed during simple refraction. Indeed, until now, it has been necessary to have recourse to the second forces of polarization to render the first sensible. The rays, in the experiment under considera-

tion here do not undergo any ordinary refraction, but simply two successive reflections; and the lamina interposed perpendicularly to the pencil of obscure heat which passes from one mirror to the other, only reveals, so to speak, its state of polarization produced by reflection alone. Indeed this species of calorific polarization may be separately developed by more direct means, exactly similar to those employed to exhibit the analogous phænomenon of light; but to do this would hazard displacing the source or the thermoscope in giving the perpendicular direction to the two planes of reflection, for it may be objected that the calorific rays do not present themselves at the opening of the thermoscopic tube, with the same directions that they affect when the two planes of reflection are parallel; or, that the intensity of the source, or its position in relation to the mirrors, has been altered during the necessary movement.

But let us return to the piles. When the planes of refraction are perpendicular, the interposition of the circular disc of mica between the two series of laminæ, increases the calorific transmission if its principal section be inclined 45° upon the first plane of refraction, and leaves it in nearly its natural state if the disc present its principal section parallel to that plane. According to the denominations adopted in England, Mr. Forbes calls the relation of the quantities of heat transmitted through the system, in these two positions of the lamina, the *effect of depolarization*. When endeavouring to determine a similar relation for heat proceeding from different sources, Mr. Forbes found that it varies even when employing the same *depolarizing* lamina, and the same system of piles arranged *under a constant inclination*. Thus, in certain circumstances, the heat of copper at 400° gave him, as the mean of several observations, 100 : 118; and the heat of incandescent platina 100 : 134. He thence concludes that calorific rays are *more or less depolarizable* *, according to their proper nature.

If the tenor of the reasoning with which this second part of the memoir commenced has been well understood, it will be easily seen that Mr. Forbes's conclusion is inadmissible. Indeed we have seen that in the conditions of distance which he adopted, the heat proceeding from the whole of the system of mica was mixed in a sensible manner with the direct rays of the source

* Trans. of the R. S. of Edin. vol. xiii., part i. p. 155, [or Lond. and Edinb. Phil. Mag. vol. vi. p. 286. Edit.]

which immediately traversed the laminæ. In each of the sources employed, the heating of the piles, and, consequently, the quantity of proper heat which they radiate upon the thermoscopic body, does not alter in the two positions which are successively given to the principal section of the interposed lamina. However, the calorific absorption of the mica, whence this heating is derived, varies with the quality of the incident rays, and becomes strong in proportion to the intransmissibility by the system, of the heat supplied from the source. Besides, we shall see that all the calorific rays undergo the same *effect of depolarization*, and consequently give the same difference between the two portions of heat that immediately traverse the system, when the principal section is parallel, and afterwards inclined the same [angular] quantity upon the plane of primitive polarization. But it is evident that by adding a given number to two different quantities, they must necessarily approach to equality, and that in a proportion corresponding to the largeness of the number added. Wherefore the heat from sources at low temperatures, that is, heat from sources whose rays are not very transmissible by mica, undergoing a greater absorption, must have produced, in Mr. Forbes's experiments, a *depolarization* smaller in *appearance* than the heat from sources at elevated temperatures, whose rays communicate less heat to the system.

I demonstrate the *equality of the depolarization* of heat of every kind by means perfectly analogous to those which I employ to prove the *equality of their polarization*.

If the subject under investigation be heterogeneous calorific fluxes transmitted by different bodies submitted to the radiation of flame, I take those endowed with the most opposite diathermancy, which, combined separately with the system of depolarization, transmit equal quantities of heat, when the principal section of my circular lamina is parallel or perpendicular to the plane of primitive polarization, and in each case I incline the principal section 45° upon this plane; the progress of the galvanometrical index is precisely the same for both experiments.

If we desire to verify this equality relatively to the heat emitted by different sources, the maximum transmission obtained with the source at a low temperature is to be first observed, and then glass laminæ, more or less thick, are to be interposed upon the exterior passage of the radiation from the source at an elevated temperature, until the effect of the minimum transmission

be equal to that observed upon the preceding source. We then pass to the augmentations produced in both cases, by inclining the principal section 45° upon the plane of refraction of the anterior pile; these two augmentations are again respectively equal.

In all these experiments the indicating needle of the galvanometer moves over a considerable space, for we have recently seen that it sometimes describes arcs which exceed 30°. The smaller angle described by virtue of the alteration of the direction of the principal section is due to the action of the heat thrown off by copper at 400°, which scarcely propels the needle beyond 7°; but as, by means of the artifice just indicated, the heat of flame may be employed to a sufficient extent to give precisely the same movement, the equality of the depolarization in these two extreme cases is proved in the most evident manner.

The two pencils of light produced by the plate of mica or sulphate of lime, in positions in which the principal section is inclined 45° upon the primitive plane of polarization interfere when they are reflected together by the second mirror, or transmitted by the second pile, and thus develope the beautiful colours treated of above. Is there an analogous interference of the calorific rays?

Coloration being here the criterion of the interference, I at first thought that I should easily succeed in verifying the existence of this phænomena in heat by experiments of diathermancy. I will endeavour to explain my ideas more clearly.

We know that the two coloured images obtained by the interposition of the plate of mica or sulphate of lime, having the principal section inclined 45° upon the plane of primitive polarization, whilst the second plane of polarization is rendered alternatively parallel or perpendicular to the first, have always complementary tints. We will suppose that these tints are the red and the green. If we view these two images produced thus successively through a glass of a very pure red colour, the first will be seen and not the second. If, instead of red, white or some other kind of coloured glass be employed, the two images will be seen, sometimes in their natural state, and sometimes altered; the red image more than the green, or the green more than the red, according to the nature of the screen glass interposed.

Would not these different alternations, produced in the relative energy of the two images, by the interposition of a given screen, be equally sensible to us, if our eyes lost the faculty of distinguishing colours, only retaining a perception of luminous in-

tensity? Now the sense of vision, reduced to this state of simplicity, would become, as to light, what our thermometers are as to heat. Wherefore, if the two *complementary pencils of obscure heat* were transmitted by a substance possessing a high degree of diathermancy, it is very possible that they might not be equally absorbed, in which case we should have an indirect proof of the interference of the two calorific pencils. I have tried the experiment with several sorts of plates, and have always obtained the same relation of transmission in the two cases. These results do not decide the question negatively. It is very possible, I will even say probable, reasoning from analogy, that the calorific rays interfere; but, in my opinion, we have not yet a single fact whence any experimental proof whatever, direct or indirect, of these interferences, may be deduced.

As to the polarization of heat, its existence and its general laws appear to me to be fully proved by the numerous facts recorded in this memoir. I have endeavoured to describe the fundamental experiments as clearly as possible, in order that all who are interested in the progress of physics may study them with facility. I may add, they are neither difficult nor uncertain; I have repeated them very many times, and in the presence of several physicists, and always with perfect success.

At the commencement of these researches we proposed to explain the contradictions presented in the results obtained by different experimenters upon calorific polarization; but this task becomes needless after the long investigation, into which we found it necessary to enter in relation to Mr. Forbes's experiments.

All the differences observed in the polarization of heat developed by the forces of reflection and refraction, are attributable to the MORE OR LESS SENSIBLE HEATING of the apparatus of polarization; excepting in the case of the tourmalines, which render the phænomena of polarization sensible or not, according to the quality of those minerals.

The portion of heat regularly reflected by the mirrors, and refracted or transmitted immediately by the piles, is very small, relatively to the quantity of heat absorbed by the mirrors or laminæ. If the thermoscopic body be placed so as to be simultaneously affected by these two species of heat, the difference existing between the feeble reflected or refracted rays in the

two positions, parallel or perpendicular, of the planes of polarization, is *concealed* by the enormous quantity of heat which the polarizers radiate upon the thermoscope equally in both cases. The manifestation of this difference commences, if the action of the secondary radiation of the mirrors or piles upon the thermoscope be comparatively feeble to that of the calorific pencil, which undergoes immediate reflection or transmission. Lastly, it attains its normal state, when, by a suitable arrangement of the apparatus, the thermoscope is completely secured from the sensible effect of this radiation, whilst left exposed to the action alone of reflected or refracted heat.

If we take a general survey of the whole of the facts which, at the present day, compose the science of radiant heat, it will be seen that this agent is propagated, reflected, refracted, and polarized, in absolutely the same manner as light. If these properties often remain unperceived, it is to be attributed to a defect of diathermaneity in the greater number of bodies, or to the particular manner, according to which their absorption is manifested upon the radiation of heat.

Some media, such as air and rock-salt, transmit equally all sorts of calorific or luminous rays; but others act in a different manner upon the rays of the two agents, extinguishing sometimes more light than heat, at others more heat than light. We have thus the singular spectacle of bodies which completely absorb the luminous rays, and admit the passage of certain calorific rays; and of substances permeable to light, completely arresting every species of heat.

Analogous differences are produced in the diffuse reflection which the two radiations experience at the surface of opake and athermanous bodies; for perfectly white substances reflect or absorb extremely diverse proportions of heat, according to the quality of the calorific rays; and yet the same white surfaces absorb all the rays of light in equal proportions. It is deducible even, with the clearest evidence, from the absence of any coloration whatever, which would not fail to appear when these surfaces were exposed to ordinary light, if, by a difference of absorption, the coloured rays, which enter into the composition of light irregularly reflected, had not between them exactly the same relations of intensity of the incident rays.

Other inequalities, also deriving their origin from absorption, are manifested in the phænomena of polarization presented by

tourmalines. In these phænomena the two pencils into which a ray of light is divided, in penetrating into the interior of the plates, are so modified in their progress, that the ordinary pencil is completely absorbed during its passage, and the extraordinary pencil presents itself alone completely polarized at the emergence; and that whatever be the colour of the incident light. The case is different with radiant heat, the two pencils of which produced at the entrance of the same polarizing plates, undergo absorptions sometimes extremely diverse, sometimes perfectly equal, which occasions great variations in the appearances of polarization, according to the quality of the calorific rays.

Polarization becomes equal for radiations of every kind, if it be produced by the forces of refraction and reflection, which are perfectly independent of the absorption of the media.

It is similar when this latter force has no longer any influence upon the phænomenon of reflection. Indeed, we have just seen above, that diffuse reflection, in which absorption acts a part so important, varies considerably from one ray of heat to the other; but the portion of incident radiation, which is reflected in a regular manner at the polished surface of rock salt, and other diaphanous substances, is equal for every species of heat and light.

All bodies exposed to radiant heat become hot, and, when withdrawn from the action of radiation, preserve for some time the heat acquired; but very few substances, after exposure to light, retain it so as to become luminous in darkness: in general the light disappears even at the moment of absorption.

In short, the heat absorbed is found, so to speak, to have changed its nature. It then forms a homogeneous flux, and the mode of its transmission acquires characters quite opposite to those effected by calorific or luminous radiation. This absorbed heat makes its way, in the body, in every direction, is propagated in it slowly, like heat communicated by contact, and its propagation is considerably modified by the displacement of the different parts, of which the body is composed. Light, and radiant heat, on the contrary, are composed of heterogeneous fluxes, they move only in a rectilinear direction, travelling over any interval whatever in an imperceptible space of time, and do not receive any influence from the agitation, whether more or less violent, of the media which transmit them.

In conclusion, these two great agents of nature, and the modifications which they undergo from the action of ponderable matter, are governed by similar laws, while their rays move freely. Numerous differences are manifested as soon as the progress of the two radiations suffers any interruption whatever, either at the surface, or in the interior of bodies.

Article V.

General Theory of Terrestrial Magnetism. By Professor Carl Friedrich Gauss, *of the University of Göttingen.*

[Translated by Mrs. Sabine, and revised by Sir John Herschel, Bart.]

The unwearied zeal with which, in recent times, endeavours have been made to examine the direction and intensity of the magnetic force of the earth, at all parts of its surface, is the more worthy of admiration, as it has been prompted by the pure love of science. Great as is the importance to navigation of the most complete attainable knowledge of the lines of declination, more than this is scarcely required for its purposes. Whilst science delights to render such useful services, her own requisitions have a wider scope, and make it necessary that equal efforts should be devoted to the examination of all the magnetic elements.

It has been customary to represent the results of magnetic observations by three systems of lines, usually termed Isogonic, Isoclinal, and Isodynamic lines. In course of time these lines undergo considerable alterations both in position and in figure, so that a drawing of them represents the phænomena correctly only for the epoch to which it corresponds. Halley's Chart of Declination for 1700 is very different from that of Barlow for 1833; and already Hansteen's Dip Chart for 1780 differs greatly from the present position of the Isoclinal lines. Doubtless, in course of time, similar alterations in the lines of intensity will be manifested; but observations of this nature are altogether too recent to furnish such indications at present.

In all these maps there exist spaces either blank, or in which the lines are but indifferently supported by observation. The inaccessibility of parts of the earth's surface renders perfection in this respect impossible; but a rapid progress towards it may be confidently hoped for.

Viewed from the higher grounds of science even a complete representation of the phænomena after this manner is not itself the final object sought. It is rather analogous to what the astronomer has accomplished, when, for example, he has observed the apparent path of a comet in the heavens. Until the complicated phænomena have been brought in subjection to a common principle, we have only building-stones, not an edifice.

The astronomer, after the comet has disappeared from his view, begins his chief employment, and resting on the laws of gravitation, calculates from the observations the elements of its true path, and is thus enabled to predict its future course. And in like manner the magnetician proposes to himself as the object of his research, as far as the different and in some respects less favourable circumstances permit,—the study of the fundamental causes which produce the phænomena, their magnitude and their mode of operation,—the subjection of the observations, as far as they extend, to those elementary principles,—and the anticipation, with some approximation at least, of their effects, in those regions where observation has not yet penetrated. It is at least well to keep in view this higher object, and to endeavour to prepare the way for it, even though the great imperfection of the data may render its attainment impossible at present.

It is not my purpose here to notice the earlier fruitless attempts to explain the enigma of these phænomena by hypotheses having no physical foundation. A physical foundation can only be allowed to such attempts as have considered the earth as a real magnet, and have employed in the calculation only the demonstrated mode of action of a magnet operating at a distance. All attempts of this nature hithert omade have this in common;—that instead of first examining what the conditions, whether simple or complex, of this great magnet must be to satisfy the phænomena, certain determinate and simple conditions were presupposed, and the subject of inquiry has been the accordance or non-accordance of the phænomena with these presupposed conditions. We see here a repetition of what has often occurred in the early history of astronomy and of other sciences.

The simplest hypothesis of this kind is that which supposes a very small magnet in the centre of the earth; or rather (as it is not likely that any one ever believed in the actual existence of such a magnet) supposes magnetism to be so distributed in the earth, that its collective action at and beyond the surface is equivalent to the action of an imaginary infinitely small magnet; much as gravitation towards a homogeneous sphere is equivalent to the attraction of a sphere of equal mass condensed in its central point. In the supposed case, the magnetic poles are the two points where the prolonged axis of the little central magnet intersects the earth's surface; where the magnetic needle is vertical and the intensity is also greatest. In the great circle midway be-

tween these two poles called the magnetic equator, the dip is $=o$ and the intensity is half as great as at the poles; between the magnetic equator and either pole, both the dip and the intensity depend on the distance from the said equator (which distance is termed the magnetic latitude) in such manner, that the tangent of the dip is equal to twice the tangent of the magnetic latitude. Lastly, the direction of the horizontal needle must everywhere coincide with the direction of a great circle drawn through the northern magnetic pole.

There is in nature only a rude approximation to all these necessary consequences of the above hypothesis. In reality the line of no dip is not a great circle, but a line of double flexure; equal intensities do not correspond to equal dips; the directions of the horizontal needle are far from all converging to one point; and so on. A very slight consideration is sufficient therefore to show the inadmissibility of this hypothesis.

One of the above propositions is however still employed as an approximation in deducing the line of no dip from observations of dips of small amount made at some little distance from it.

About eighty years ago, Tobias Mayer used a similar hypothesis, but with this modification; that instead of supposing the infinitely small magnet at the centre of the earth, he placed it at about the seventh part of the earth's radius from the centre; at the same time (probably in order to avoid greater complication in the calculations) he retained the wholly arbitrary supposition, of the plane perpendicular to the axis of the magnet passing through the centre of the earth. In this manner, on a comparison of the observed variations and dips, at a very small number of places it is true, he found them agree very well with his calculation. A more extended comparison would have shown that this hypothesis did not afford a much better representation than the first-mentioned one, of the whole phænomena of the dip and declination. No observations of the intensity had been at that time made, at least as far as we know.

Hansteen went a step further, by the endeavour to represent the phænomena on the hypothesis of two infinitely small eccentric magnets of unequal strength. The decisive test of an hypothesis must always be the comparison of its results with those of experiment. Hansteen compared his with observations at forty-eight different places, amongst which however there were only twelve at which the intensity had been determined, and

only six complete in the three elements. In these comparisons we find in the dip differences of 13° between calculation and observation*.

If these differences are greater than are admissible in a satisfactory theory, one cannot avoid drawing the conclusion, that the magnetic conditions of the earth are not such as to admit of representation by means of a concentration in either one or two infinitely small magnets. It is not denied that with a greater number of such fictitious magnets, a sufficient agreement might be ultimately attainable; but how far such a mode of solving the problem might be advisable is quite a different question. The calculations are extremely laborious even with two magnets; with an increased number they would probably present insuperable difficulties. It will be best to abandon entirely this mode of proceeding, which reminds one involuntarily of the attempts to explain the planetary motions by continued accumulation of epicycles.

In the present treatise it is my purpose to develope the general theory of terrestrial magnetism independently of all particular hypotheses as to the distribution of the magnetic fluids in the body of the earth; and to communicate the results which I have obtained from the first application of the method. Imperfect as these results must be, they give an idea of what may be hoped for in future, when trustworthy and complete observations from all parts of the earth shall be obtained, and employed in renewed and more refined attempts.

1.

The force which at each part of the earth imparts a certain direction to a magnetic needle suspended by its centre of gravity, (supposing it free from all extraneous influence, such, for example, as that of another artificial magnet, or the conductor of a galvanic current,) is termed the earth's magnetic force, in so far as the source whence it is derived is to be sought for in the earth itself. It may indeed be doubted, whether the seat of the proximate causes of the regular and irregular changes which are hourly taking place in this force, may not be regarded as exter-

* In the declination there is even a difference in one instance of 29 degrees; but it is proper to estimate the error of the calculation, not by the number of degrees of declination, but by the true angular difference between the calculated and observed directions, which in the case in question is $11\frac{1}{2}$ degrees.

nal in reference to the earth. We may hope, that from the general attention now directed to these phænomena, much light may shortly be thrown upon their causes. But it should not be forgotten that these changes are comparatively very small, and that there must therefore exist a much more powerful and constantly acting principal force, of which we assume the seat to be in the earth itself. A consequence which follows from this consideration is, that the facts which are to serve as the foundation on which the study of the principal force must be based, ought properly themselves to be first freed from the effects of the anomalous changes. This can only be done by mean values, drawn from numerous and continued observations; and until we shall possess such purified results, from a great number of stations distributed over the whole surface of the globe, the utmost that can be looked for is an approximation, in which there must still remain differences of the order of these anomalies.

2.

The foundation of our researches is the assumption, that the terrestrial magnetic force is the collective action of all the magnetized particles of the earth's mass. We represent to ourselves magnetization as a separation of the magnetic fluids. Admitting this representation, the mode of action of the fluids (repulsion of similar and attraction of dissimilar particles inversely as the square of the distance) belongs to the number of established physical truths. No alteration in the results would be caused by changing this mode of representation for that of Ampère, whereby, instead of magnetic fluids, magnetism is held to consist in constant galvanic currents in the minutest particles of bodies. Nor would it occasion a difference if the terrestrial magnetism were ascribed to a mixed origin, as proceeding partly from the separation of the magnetic fluids in the earth, and partly from galvanic currents in the same; inasmuch as it is known, that for each galvanic current, may be substituted such a given distribution of the magnetic fluids in a surface bounded by the current, as would exercise in each point of external space precisely the same magnetic action as would be produced by the galvanic current itself.

3.

For the measurement of the magnetic fluids we take, as in

the *Intensitas Vis Magneticæ, &c.*, for our positive fundamental unity, that quantity of northern fluid which at the unit of distance exercises on an equal quantity of the same fluid a moving force equivalent to what we assume as unity.

When we speak of the magnetic force which in any point of space is produced by the action of the magnetic fluid elsewhere, we always mean to speak of the moving force which is there exercised on the unity of the positive magnetic fluid; therefore in this sense the supposed magnetic fluid μ concentrated in a point exercises at the distance ρ the magnetic force $\frac{\mu}{\rho^2}$, of either repulsion or attraction in the direction ρ, according as μ is positive or negative. Representing by a, b, c, the co-ordinates of μ in relation to three rectangular axes, and by x, y, and z, the co-ordinates of the point where the force is exercised, so that

$$\rho = \sqrt{([x-a)^2 + (y-b)^2 + (z-c]^2)};$$

and resolving the force in parallels to the co-ordinate axes, the components are

$$\frac{\mu(x-a)}{\rho^3}, \frac{\mu(y-b)}{\rho^3}, \frac{\mu(z-c)}{\rho^3},$$

which, as is easily seen, are equal to the partial differential coefficients of $-\frac{\mu}{\rho}$ relatively to x, y, and z.

If besides μ, there are also in operation other portions of the magnetic fluids μ', μ'', μ''', &c., concentrated in points, of which the distances from the spot where the force is exercised are ρ', ρ'', ρ''', &c., then the components of the whole resulting magnetic force, parallel to the co-ordinate axes, are equal to the partial differential co-efficients of

$$-\left(\frac{\mu}{\rho} + \frac{\mu'}{\rho'} + \frac{\mu''}{\rho''} + \frac{\mu'''}{\rho'''} + \text{\&c.}\right),$$

relatively to x, y, and z.

4.

Hence may easily be shown what magnetic force is exercised in each point of space by the earth, however the magnetic fluids may be distributed therein. Imagine the whole volume of the earth, as far as it contains free magnetism (that is to say, separated magnetic fluids), to be divided into infinitely small elements; designate generally the quantity of free magnetic fluid

contained in each of these elements by $\mathrm{d}\,\mu$, in which the southern fluid is always considered as negative; call ρ the distance of $\mathrm{d}\,\mu$ from a point in space, the rectangular co-ordinates of which may be x, y, z; lastly, let V denote the aggregate of $\frac{\mathrm{d}\,\mu}{\rho}$ comprehending with reversed signs the whole of the magnetic particles of the earth: or say

$$V = -\int \frac{\mathrm{d}\,\mu}{\rho}.$$

Thus V has in each point of space a determinate value, or it is a function of x, y, z, or of any other three variable magnitudes, whereby we may define points in space. We then obtain, by the following formulæ, the magnetic force ψ in every point of space, and the components of ψ, parallel to the co-ordinate axes, which we shall call ξ, η, ζ,

$$\xi = \frac{d\,V}{d\,x},\ \eta = \frac{d\,V}{d\,y},\ \zeta = \frac{d\,V}{d\,z},\ \psi = \sqrt{(\xi^2 + \eta^2 + \zeta^2)}.$$

5.

I shall first develope some general propositions which are independent of the form of the function V, and are worthy of attention from their simplicity and elegance.

The complete differential of V becomes

$$\begin{aligned} d\,V &= \frac{d\,V}{d\,x}\,.\,d\,x + \frac{d\,V}{d\,y}\,.\,d\,y + \frac{d\,V}{d\,z}\,.\,d\,z\,. \\ &= \xi\,d\,x + \eta\,d\,y + \zeta\,d\,z. \end{aligned}$$

If we designate by $d\,s$ the distance between the two points to which V and $V + d\,V$ belong, and by θ the angle which the direction of the magnetic force ψ makes with $d\,s$, we have

$$d\,V = \psi \cos\theta\,.\,d\,s,$$

because as $\frac{\xi}{\psi}, \frac{\eta}{\psi}, \frac{\zeta}{\psi}$ are the cosines of the angles which the direction of ψ makes with the co-ordinate axes, so $\frac{d\,x}{d\,s}, \frac{d\,y}{d\,s}, \frac{d\,z}{d\,s}$, are the cosines of the angles between $d\,s$ and the same axes.

Therefore $\frac{d\,V}{d\,s}$ is equal to the force resolved in the direction of $d\,s$; the same follows from the equation $\frac{d\,V}{d\,x} = \xi$ if we bear in mind that the co-ordinate axes may be arbitrarily chosen.

6.

If two points in space P^0, P', be connected by an arbitrary line, of which $d\,s$ represent an indeterminate element, and if, as before, θ signify the angle between $d\,s$ and the direction of the magnetic force there existing, and ψ its intensity, then

$$\int \psi \cos \theta \,.\, d\,s = V' - V^0$$

if we extend the integration through the whole line, and designate by V^0, V', the values of V at the extremities.

The following corollaries of this fruitful proposition deserve especial notice:—

I. The integral $\int \psi$ cos. $\theta \,.\, d\,s$ preserves the same value by whatever path we proceed from P^0 to P'.

II. The integral $\int \psi \cos \theta \,.\, d\,s$, extended through the whole length of any re-entering curve, is always $= 0$.

III. In a re-entering curve, if θ is not throughout $= 90°$, a part of the values of θ must be greater and a part must be less than 90°.

7.

Those points of space in which V has a value greater than V^0, are divided from those in which the value of V is less than V^0, by a surface in all the points of which V has one determinate value $= V^0$*.

It follows from the proposition in Art. V., that in each point of this surface the magnetic force has a direction perpendicular to the surface, and *towards* the side where the higher values of V are found. Let $d\,s$ be an infinitely small line perpendicular to the surface, and $V^0 + d\,V^0$ the value of V at its other extremity; then the intensity of the magnetic force will be $= \frac{d\,V^0}{d\,s}$. The series of points for which $V = V^0 + d\,V^0$, form a second surface infinitely near to the first, and at different points in the whole intervening space the intensity of the magnetic force is in the inverse ratio of the distance apart of the two surfaces.

* If the function V could have any arbitrarily chosen form, then in particular cases a maximum or a minimum value of V might correspond to an insulated point, or to an insulated line, around which only greater or only less values might be found, or it might correspond to a surface on *both* sides of which there might be greater or on both less values. But the conditions to which the function V is subjected do not allow the occurrence of such excepted cases. A full development of this subject, as it is unnecessary for our present object, must be reserved for another occasion.

Let V alter by infinitely small but equal steps. A system of surfaces will be produced, dividing space into infinitely thin strata, and the inverse ratio of the thickness of the strata to the intensity of the magnetic force will then hold good not only for different points in one and the same stratum, but also for different strata.

8.

We will now take into consideration the values of V on the surface of the earth.

At a point P of the earth's surface let ψ be the intensity; $P\,M$ the direction of the whole magnetic force; ω the intensity, and $P\,N$ the direction of the force projected on the horizontal plane, or $P\,N$ the direction of the magnetic meridian, meaning thereby the direction indicated by the north pole of the magnetic needle; i the angle between $P\,M$ and $P\,N$, or the dip; θ, t, the angles formed by the elementary portion $d\,s$ of a line on the surface of the earth and the directions $P\,M$, $P\,N$. Lastly, V and $V + d\,V$ correspond to the two extremities of $d\,s$.

We have consequently

$$\cos\theta\,. = \cos i \cos t,\ \omega = \psi \cos i.$$

And the equation in Art. V. becomes

$$d\,V = \omega \cos t\,.\,d\,s$$

If two points on the earth's surface P^0 and P', at which V has the value of V^0 and V', are connected by a line traced on the surface of the earth of which $d\,s$ is an indeterminate element, then

$$\int \omega \cos t\,.\,d\,s = V' - V^0,$$

if the integration be extended through the whole line; and it is plain that three corollaries hold good similar to those in Art. VI., namely,

I. That the integral $\int \omega \cos t\,.\,d\,s$ keeps the same value by whatever path you proceed on the surface of the earth from P^0 to P'.

II. The integral $\int \omega \cos t\,.\,d\,s$ throughout the whole length of a closed line on the surface of the earth is always $= 0$.

III. In such a closed line, unless throughout its course $t = 90°$, a part of the values of t must necessarily be acute and a part obtuse.

9.

Propositions I. and II. of the foregoing article (which, pro-

perly speaking, are only different modes of expressing the same thing) may be tested, at least approximately, by a reference to observation.

Let P^0, P', $P'' \ldots . P^0$ be a polygon on the surface of the earth, the sides of which are the shortest lines that can be drawn between their respective extremities, and are therefore portions of great circles, the earth being here considered simply as a sphere. Let ω^0, ω', ω'', &c. be the intensities of the horizontal magnetic force at the points P^0, P', P'', &c.; further, let δ^0, δ', δ'', &c. be the declinations reckoned in the usual manner, west of north as positive, east of north as negative; lastly, let (01) be the azimuth of the line $P^0\,P'$ at P^0, reckoned in the customary manner, from the south by the west; in like manner (10) the azimuth of the same line taken backwards at P', and so on.

Let it be observed that t alters continuously in each of the sides of the polygon, but suddenly at the corners, where therefore ithas two different values; for example, at P, t has the value $(10) + \delta'$, in consideration that P' is the end of the line $P^0\,P'$; and the value of $18^0 + (12) + \delta'$, in regard that it is the beginning of $P'\,P''$.

We may consider the approximate value of the integral $\int \omega \cos t \,.\, d\,s$, extended through $P^0\,P'$, to be

$$\tfrac{1}{2}\,(\omega^0 \cos t^0 + \omega' \cos t') \,.\, P^0\,P',$$

where t^0 and t' signify the values of t at P^0 as the beginning, and at P' as the end of $P^0\,P'$. This approximation is all that can be obtained, because we have the values of ω and t only at the extremities $P^0\,P'$, and is deserving of confidence in proportion to the shortness of the line. The given expression is, in our notation,

$$= \tfrac{1}{2}\,(\omega' \cos ((10)) + \delta') - \omega^0 \cos ((01)) + \delta^0]) \,.\, P^0\,P'.$$

In like manner, the approximate value of the integral, extended through $P'\,P''$, is

$$= \tfrac{1}{2}\,(\omega' \cos ((21) + \delta'') - \omega' \cos ((12) + \delta']) \,.\, P'\,P'',$$

and so on through the whole polygon.

Therefore, for a triangle our proposition gives the approximatively correct equation

$$\begin{aligned}&\omega^0\,(P^0\,P' \cos ((01) + \delta^0) - P^0\,P'' \cos ((02) + \delta^0])\\ &+ \omega'\,(P'\,P'' \cos ((12) + \delta') - P^0\,P' \cos ((10) + \delta'])\\ &+ \omega''\,(P^0\,P'' \cos ((20) + \delta'') - P'\,P'' \cos ((21) + \delta'']) = 0.\end{aligned}$$

It is obvious that in this equation the units of intensity and of distance are arbitrary.

10.

As an example, we will apply the formula to the magnetic elements of

Göttingen	$\delta^0 = 18°\ 38'$	$i^0 = 67°\ 56'$	$\psi^0 = 1{\cdot}357$
Milan	$\delta' = 18\ \ 33$	$i' = 63\ \ 49$	$\psi' = 1{\cdot}294$
Paris	$\delta'' = 22\ \ 04$	$i'' = 67\ \ 24$	$\psi'' = 1{\cdot}348$

whence it follows that $\omega^0 = 0{\cdot}50980$

$$\omega' = 0{\cdot}57094$$

$$\omega'' = 0{\cdot}51804.$$

Taking the geographical position of

Göttingen	51° 32′ latitude	9° 58′ longitude from Greenwich
Milan	45 28	9 09
Paris	48 52	2 21

and performing the calculation for a spherical surface only, we find

$$\left.\begin{array}{l}(01) = \ \ \ 5°\ 11'\ 31'' \\ (10) = 184\ \ 35\ \ 35\end{array}\right\} P^0 P' = 6°\ \ 5'\ 20''$$

$$\left.\begin{array}{l}(12) = 128\ \ 47\ \ 31 \\ (21) = 303\ \ 48\ \ 01\end{array}\right\} P' P'' = 5\ \ 44\ \ 06$$

$$\left.\begin{array}{l}(20) = 238\ \ 20\ \ 20 \\ (02) = \ \ 64\ \ 10\ \ 12\end{array}\right\} P^0 P'' = 5\ \ 32\ \ 04$$

Substituting these values in our equation, and those given above for δ°, δ', δ'', we have

$$0 = 17556\ \omega^0 + 2774\ \omega' - 20377\ \omega'',$$

$$\text{or, } \omega'' = 0{\cdot}86158\ \omega^0 + 0{\cdot}13613\ \omega'.$$

Hence we deduce from the observed horizontal intensities at Göttingen and Milan, that at Paris $\omega'' = 0{\cdot}51696$, agreeing almost exactly with the observed value 0·51804.

It is easily seen that if we permit ourselves to take the distances P^0, P'', &c. instead of their sines, the above formula can be expressed immediately by the geographical longitudes and latitudes of the places.

11.

The line on the earth's surface, in all points of which V has the same value $= V^0$, divides generally speaking the parts of the surface in which the value of V is greater than V^0, from those in

which it is less. The direction of the horizontal magnetic force in each point of this line is obviously perpendicular to it, and towards the side where the greater values of V are found. If ds be an infinitely small line in this direction, and $V^0 + dV^0$ the value of V at the other extremity of this line, then $\frac{dV^0}{ds}$ is the intensity of the horizontal magnetic force at this place. As here also the series of points corresponding to the value of $V = V^0 + dV^0$ forms a second line situated infinitely near to the first, and thus marks out on the surface of the earth a *zone*, within which the values of V are between V^0 and $V^0 + dV^0$, and where the horizontal intensity is in an inverse ratio to the breadth of the zone; so by making V vary by infinitely small but equal steps from the lowest value on the surface of the earth to the highest, the whole surface of the globe becomes divided into an infinite number of infinitely narrow zones, the direction of the horizontal magnetic force being everywhere perpendicular to the dividing lines, and its intensity being in an inverse ratio to the breadth of the zone at the place in question. The two extreme values of V correspond in this point of view to two points, inclosed by the zones, at which the horizontal force is $= 0$, and where therefore the whole magnetic force can only be vertical: these points are termed the magnetic poles of the earth. The lines dividing the zones are no other than the intersections of the surfaces considered in Article VII. with the surface of the earth, whilst it is only at the poles that they are in contact with it.

12.

The form of the system of lines described in the above article is strictly but the simplest type, which might be subject to many exceptions were we to take into account every possible distribution of magnetism in the earth. We shall not, however, exhaust this subject here, but shall only add a few elucidatory remarks as to the cases of exception. The magnetic condition of the earth, no doubt is such, that the form of the system of lines on its surface corresponds to the description. At least there are certainly no exceptions on the great scale, though probably there may occur local ones. Some philosophers have considered the earth as having two north and two south magnetic poles, but it does not appear that an essential condition was previously ful-

filled, by a *precise* definition being given of what should be understood by a magnetic pole. We intend to apply this denomination to each point of the earth's surface where the horizontal intensity $= 0$: where therefore, speaking generally, the dip $= 90$; but including the singular case, did it exist, where the total intensity $= 0$. If we were to give the name of magnetic poles to those places where the total intensity is a maximum (*i. e.* greater than anywhere in the surrounding vicinity), it must not be forgotten that this is something quite different from the above definition; that neither the situation nor the number of these last-named points have any necessary connexion with those of the points first spoken of; and that it tends to confusion when dissimilar things are called by the same name. If we look away from the actual condition of the earth and take the question in its generality, there may certainly exist more than two magnetic poles; but it does not appear to have been noticed that if, for example, two north poles exist, there must necessarily be between them yet a third point, which is likewise a magnetic pole, but is properly neither a north nor a south pole, or is both if that expression be preferred. A consideration of our system of lines will best serve to elucidate this subject. If the function V have at a point of the earth's surface P^* a maximum value V^*, and all around smaller values, then a series of progressively decreasing values will correspond to a system of rings, each of which will inclose all the preceding ones, together with the point P^*, and on each of these rings the direction of the horizontal magnetic force, or that of the north pole of the magnetic needle, will be *inwards*†.

This is the characteristic mark of a magnetic north‡ pole.

It is clear that the rings may be made so small, or the corresponding values of the function V may differ so little from V^*, that any other point may be excluded.

We will designate by S the space included by all the points on the surface of the earth at which the value of V is greater

† These rings, themselves assumed as infinitely small, are not necessarily circular, but generally speaking oval, so that the normal direction of the magnetic needle in reference to them only coincides with the direction towards P^* at four points of each ring. Great error may be involved, therefore, if without further precaution, the intersection of the prolongations of two compass directions at considerable distances is assumed to be P^*.

‡ We conform here to the mode of speaking in common usage, according to which the point established by Captain James Ross is so designated, although properly speaking it is a south pole, when the earth itself is considered as a magnet.

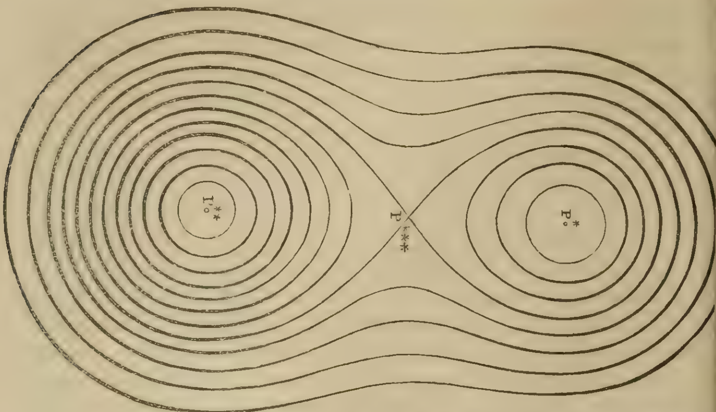
than a given value W. It is clear that S may either be one connected surface or several detached spaces, and that $V = W$, *on* the bounding lines or lines which separate S from other parts where V is less than W; by increasing or diminishing W, we enlarge or contract the space S.

Now let us assume P^{**} to be a second point of similar properties to P^* so that at it also V may have a maximum value $= V^{**}$. As according to what has been before noticed, W may have a value less than V^*, and differing from it by so small an amount that P^{**} shall fall outside that part of S in which P^* is situated; then if we arrange (as we may do) that V^{**} shall not be less than V^*, it will be greater than W, and P^{**} will necessarily also belong to a part of S. Thus P^* and P^{**} will both be situated in S, but in separate portions of it. On the other hand, it is evident that W may be taken so small that P^* and P^{**} shall both be situated in one connected part of S; for by only taking W small enough, S may be made to embrace the whole surface of the earth.

If then W be made to pass progressively through all the values between the first and the second values spoken of, there must be amongst them one which we will call $= V^{***}$, characterised by being the lowest at which P^* and P^{**} are still situated in separate portions of S, which separate portions will unite whenever W is diminished further.

If this union occur at a point P^{***}, the bounding line on which $V = V^{***}$ will have the form of an 8, crossing at that point; where also we may easily satisfy ourselves that the horizontal intensity must $= 0$. In fact, the crossing either does or does not take place under an angle of sensible amount.

In the first case, the horizontal force, if it be not$=0$, must be directed in the normal to the two different tangents, which is absurd; in the second case, in which the two halves of the 8 touch each other at P^{***}, or would have the same tangent, the force normal to this tangent could only be directed towards the interior of one half surface of the 8, which involves a contradiction, as the value of V increases towards both sides; therefore P^{***} is a true magnetic pole according to our definition, but must be considered as a south pole as regards the points nearest to it inside the two openings of the 8, and as a north pole as regards the points which lie outside. Figure 1. illustrates this form of the system of lines.



If the junction take place at two different points, what has been demonstrated for one point would hold good for the two; and one may easily see that inside the space inclosing P^* and P^{**} an insular space would be formed, which would gradually contract itself as W was diminished, and would necessarily at length resolve itself into a true south pole.

The case is similar when the junction takes place at three or more separate points; but if it take place at once on a whole line, then the horizontal force must disappear on all the points of that line.

It is evident that the assumption of two south poles would in like manner necessitate the existence of a third polar point, which would be neither a south pole nor a north pole, or rather would be both at once.

13.

From what has been developed in the foregoing article, its application to many conceivable exceptions from the simplest type of our system of lines will be readily understood. The whole of the points to which a certain value of V corresponds, may be a line consisting of several portions, of which each returns back into itself, but which are quite separate from each other; it may be a line crossing itself; lastly, it may be a line having on both sides spaces where V is greater than on the line, or where it is less.

We may assert that on the earth there are, on the great scale, no deviations of such a nature from the simplest type.

Local deviations, indeed, may well be supposed to exist. Magnetic masses near the surface, though producing no sensible effect at any considerable distance, may obscure and even obliterate the regular progress of the terrestrial magnetic force in their immediate vicinity. In the simplest case the system of lines in such a district might take the form represented in Figure 2.

14.

After this geometrical representation of the relations of the horizontal magnetic force, we proceed to develope the mode of submitting them to calculation. On the surface of the earth V becomes a simple function of two variable magnitudes, for which we will take the geographical longitude reckoned eastward from an arbitrary first meridian,—and the distance from the north pole of the earth; we will designate the first of these, or the longitude, by λ, and the second, or the complement of the geographical latitude, by u. Considering the earth as a spheroid of revolution, of which the greater semi-axis $= R$, and the lesser semi-axis $= (1-\epsilon)\, R$, an element of the meridian is

$$= \frac{(1-\epsilon)^2 R \,.\, du}{(1-(2\,\epsilon-\epsilon^2)\cos u^2)}^{\frac{3}{2}};$$

and an element of the parallel is

$$= \frac{R \sin u \,.\, d\lambda}{\sqrt{(1-(2\,\epsilon-\epsilon^2)\cos u^2)}}$$

Resolving the horizontal magnetic force into two portions, one of which, X, acts in the direction of the geographical meridian, and the other, Y, perpendicularly to that meridian,—and considering X as positive when directed towards the north, and Y as positive when directed towards the west,—then

$$X = -\frac{(1-(2\epsilon-\epsilon^2)\cos u^2)^{\frac{3}{2}}}{(1-\epsilon)^2} \cdot \frac{dV}{R\,du}$$

$$Y = -\sqrt{(1-(2\epsilon-\epsilon^2)\cos u^2)} \cdot \frac{dV}{R\sin u.\,d\lambda}.$$

The total horizontal force is then

$$= \sqrt{(X^2+Y^2)},$$

and the tangent of the declination

$$= \frac{Y}{X}.$$

Neglecting the square of the ellipticity, ϵ, the expressions become

$$X = -(1+(2-3\cos u^2)\,\epsilon) \cdot \frac{dV}{R\,du}$$

$$Y = -(1-\epsilon\cos u^2) \quad . \quad \frac{dV}{R\sin u.\,d\lambda},$$

or, setting the ellipticity quite aside,

$$X = -\frac{dV}{R\,du}$$

$$Y = -\frac{dV}{R\sin u.\,d\lambda}.$$

The data furnished by the observations which we possess are much too scanty, and most of them much too rude, to make it advisable at present to take into account the spheroidal form of the earth. It would not be difficult to do so; but it would complicate the calculations without affording any corresponding advantage. We will therefore adhere to the last-mentioned formula, in which the earth is considered as a sphere, whose semi-diameter $= R$.

15.

If X be expressed by a given function of u and λ, Y can be be deduced from it *a priori*.

Let the integral $\int_0^u X\,du = T$, considering λ as constant in the integration: it is then clear that if we differentiate in a similar

manner according to u, $\frac{d\,(V + R\,T)}{d\,u} = o$; $V + R\,T$ having a value independent of u, or, what is the same thing, constant in all the points of a meridian,—it must hence also be absolutely constant, because all meridians converge and meet at the poles.

If we call the value of V at the north pole $= V^*$, then $T = \frac{V^* - V}{R}$;

and hence $$Y = \frac{d\,T}{\sin u\,.\,d\,\lambda}.$$

This result may also be expressed as follows:

$$Y = \frac{1}{\sin u} \int_0^u \frac{d\,X}{d\,\lambda} . d\,u.$$

16.

This remarkable proposition, that, *if the component of the horizontal magnetic force directed towards the north be given for the whole surface of the earth, then the component directed towards the west (or towards the east) follows of itself*, is true, conversely, only with a certain modification. If Y be expressed by a given function of u and λ, and if U represent the indeterminate integral $\int \sin u\,.\,Y\,d\,\lambda$, u being assumed constant in the integration, then $\frac{d\,(V + R\,U)}{d\,\lambda} = o$, or $V + R\,U$ has a value independent of λ, and is, generally speaking, a function of u. Thus $\frac{d\,(V + R\,U)}{R\,d\,u} = \frac{d\,U}{d\,u} - X$ is such a function; that is to say, the formula $\frac{d\,U}{d\,u}$ gives an imperfect expression for X, a part of it containing u only remaining undetermined. This want would be supplied if, besides the expression for Y, we had also that for X, for some one given meridian, or to speak generally, for some line extending from the north to the south pole. We see therefore that, *if we know the component of the horizontal magnetic force in the direction towards the west for the whole of the earth's surface, and the component in the direction towards the north for all points of some one line extending from the north pole to the south pole, the latter component, for the whole of the earth's surface, follows of itself.*

17.

The foregoing investigations apply only to the horizontal portion of the earth's magnetic force. In order to embrace the vertical force also, we must consider the problem in all its generality; therefore V must be regarded as a function of three variable magnitudes, expressing the position in space of an undetermined point O. We select for the purpose the distance r from the centre of the earth, the angle u which r makes with the northern part of the earth's axis, and the angle λ, which a plane passing through r and the axis of the earth makes with a first meridian, counted as positive towards the east.

Let the function V be expanded into a series, decreasing according to the powers of r, and to which we give the following form:

$$V = \frac{R^2 P^0}{r} + \frac{R^3 P'}{r^2} + \frac{R^4 P''}{r^3} + \frac{R^5 P'''}{r^4}, \text{ \&c.}$$

The co-efficients P^0, P', P'', &c. are here functions of u and λ; in order to see how they are connected with the distribution of the magnetic fluid in the earth, let $\mathrm{d}\,\mu$ be an element of the earth's magnetism, ρ its distance from O, and let r^0, u^0, λ^0, signify for $\mathrm{d}\,\mu$ the same as r, u, λ for O. We have then $V = -\int \frac{\mathrm{d}\,\mu}{\rho}$ extended so as to include every $\mathrm{d}\,\mu$; further $\rho = \sqrt{}\,(r^2 - 2\,r\,r^0)\cos u \cos u^0 + \sin u \sin u^0 \cos(\lambda - \lambda^0) + r^0 r^0$, and if $\frac{1}{\rho}$ be developed in the series,

$$\frac{1}{\rho} = \frac{1}{r}\left(T^0 + T'\frac{r^0}{r} + T''.\frac{r^0 r^0}{r^2} + \text{\&c.}\right)$$

then $R^2 P^0 = -\int T^0\, d\,\mu,\ \ R^3 P' = -\int T' r^0\, d\,\mu,$

$$R^4 P'' = -\int T'' r^0 r^0\, \mathrm{d}\,\mu, \text{ \&c.}$$

As $T^0 = 1$, and as according to the fundamental supposition with which we set out, the quantities of positive and of negative fluid are equal in every measureable particle in which they exist, and therefore are equal in the *whole* earth; that is to say, $\int \mathrm{d}\,\mu = 0$, it follows that

$$P^0 = 0,$$

or the first number of our series for V goes out.

We see further that P' has the form

$$R^3 P' = a \cos u + \beta \sin u \cos \lambda + \gamma \sin u \sin \lambda,$$

where $a = -\int \cos u^0 r^0 \, d\,\mu$, $\beta = -\int \sin u^0 \cos \lambda^0 r^0 \, d\,\mu$, $\gamma = -\int \sin u \sin \lambda^0 r^0 \, d\,\mu$. Therefore, according to the explanation laid down in page 13 of the *Intensitas Vis Magneticæ*, $-a, -\beta, -\gamma$, are the moments of the earth's magnetism, in relation to three rectangular axes, of which the first is the axis of the earth, and the second and the third are the equatorial radii for longitudes 0 and 90°.

The general formulæ for all co-efficients of the series for $\frac{1}{\rho}$ may be assumed as known; it is merely necessary for our purpose to remark, that in relation to u, λ, the co-efficients are rational integral functions of $\cos u \,.\, \sin u \cos \lambda$, and $\sin u \sin \lambda$, and of T'' of the second order, T''' of the third, &c. It is the same as to the co-efficients P'', P''', &c.

The series for $\frac{1}{\rho}$, and for V, converge, so long as r is not less than R, or rather, not less than the half diameter of a sphere, which includes all the magnetic particles of the earth.

18.

The function V being composed of $-\int \frac{d\,\mu}{\rho}$, satisfies the following partial differential equation:

$$0 = \frac{r\, d^2 r\, V}{d\, r^2} + \frac{d^2\, V}{d\, u^2} + \cot u \,.\, \frac{d\, V}{d\, u} + \frac{1}{\sin u^2} \cdot \frac{d^2\, V}{d\, \lambda^2},$$

which is only transformation of the well-known equation

$$0 = \frac{d^2\, V}{d\, x^2} + \frac{d^2\, V}{d\, y^2} + \frac{d^2\, V}{d\, z^2},$$

where x, y, z signify the rectangular co-ordinates of O. If we substitute the value of V,

$$V = \frac{R^3 P'}{r^2} + \frac{R^4 P''}{r^3} + \frac{R^5 P'''}{r^4} +, \text{\&c.},$$

it is clear that for the several co-efficients, P', P'', P''', &c., there will likewise be partial differential equations, of which the general expression is

$$0 = n\,(n+1)\, P^{(n)} + \frac{d^2\, P^{(n)}}{d\, u^2} + \cot u \frac{d\, P^{(n)}}{d\, u} + \frac{1}{\sin u^2} \cdot \frac{d^2\, P^{(n)}}{d\, \lambda^2}.$$

From this equation, combined with the remark in the preceding article, we obtain the general form of $P^{(n)}$. If we represent by $P^{n,m}$ the following function of u,

$$\Big(\cos u^{n-m} - \frac{(n-m)(n-m+1)}{2(2n-1)} \cos u^{n-m-2}$$
$$+ \frac{(n-m)(n-m-1)(n-m-2)(n-m-3)}{2\cdot 4(2n-1)\quad(2n-3)} \cos u^{n-m-4} - \&c.,\Big) \sin u^{m}$$

then $P^{(n)}$ has the form of an aggregate of $2n+1$ parts,

$$P^{(n)} = g^{n,0} P^{n,0} + (g^{n,1} \cos \lambda + h^{n,1} \sin \lambda) P^{n,1} +$$
$$(g^{n,2} \cos 2\lambda + h^{n,2} \sin 2\lambda) P^{n,2} +, \&c. +$$
$$(g^{n,n} \cos n\lambda + h^{n,n} \sin n\lambda) P^{n,n},$$

where $g^{n,0}$, $g^{n,1}$, $h^{n,1}$, $g^{n,2}$, &c. are determinate numerical co-efficients.

19.

If the magnetic force at the point O be resolved into three forces perpendicular to each other, X, Y, and Z, of which Z is directed towards the centre of the earth, and X and Y are tangential to a spherical surface concentric with the earth, passing through O, X directed northwards in a plane passing through O and the axis of the earth, and Y directed westwards in a plane parallel to the equator of the earth, then

$$X = -\frac{dV}{r\,du}, \quad Y = -\frac{dV}{r \sin u\, d\lambda}, \quad Z = -\frac{dV}{dr};$$

consequently,

$$X = -\frac{R^3}{r^3}\left(\frac{dP'}{du} + \frac{R}{r}\cdot\frac{dP''}{du} + \frac{R^2}{r^2}\cdot\frac{dP'''}{du}, \&c.\right)$$
$$Y = -\frac{R^3}{r^3 \sin u}\left(\frac{dP'}{d\lambda} + \frac{R}{r}\cdot\frac{dP''}{d\lambda} + \frac{R^2}{r^2}\cdot\frac{dP'''}{d\lambda}, \&c.\right)$$
$$Z = \frac{R^3}{r^3}\left(2P' + \frac{3RP''}{r} + \frac{4R^2P}{r^2}, \&c.\right)$$

On the surface of the earth X and Y are the same horizontal components which we have designated above by those letters; Z is the vertical component, which is positive when directed downwards.

The expressions for these forces on the surface of the earth are, then,

$$X = -\left(\frac{dP'}{du} + \frac{dP''}{du} + \frac{dP'''}{du} +, \&c.\right)$$

$$Y = -\frac{1}{\sin u}\left(\frac{d\,P'}{d\,\lambda} + \frac{d\,P''}{d\,\lambda} + \frac{d\,P'''}{d\,\lambda} +, \&c.\right)$$

$$Z = 2\,P' + 3\,P'' + 4\,P''' +, \&c.$$

20.

If we combine, then, with these propositions, the known theorem, that every function of λ and u, which, for all values of λ. from 0 to 360°, and of u, from 0 to 180°, has a determinate finite value, may be developed into a series of the form

$$P^0 + P' + P'' + P''' +, \&c.$$

the general member of which, P^n, satisfies the above partial differential equation,—that such a developement is only possible in one determinate manner,—and that this series always converges,—we obtain the following remarkable propositions.

I. The knowledge of the value of V at all points of the earth's surface is sufficient to enable us to deduce the general expression of V for all external space, and thus to determine the forces X, Y, Z, not only on the surface of the earth, but also for all external space.

It is clearly only necessary for this purpose to develope $\frac{V}{R}$ into a series according to the above-mentioned theorem.

In the sequel, therefore, unless it is expressly stated otherwise, the symbol V is always to be taken as limited to the surface of the earth, or as that function of λ and u which follows from the general expression, when r is made $= R$: thus

$$V = R\,(P' + P'' + P''' +, \&c.)$$

II. The knowledge of the value of X at all points of the earth's surface is sufficient to obtain all that has been referred to in Prop. I. In fact, according to Art. 15, the integral $\int_0^u X\,d\,u = \frac{V^0 - V}{R}$, V^0 signifying the value of V at the north pole, and the developement of $\int_0^u X\,d\,u$ into a series of the form referred to must necessarily be identical with

$$V^0 - P' - P'' - P''', \&c.$$

III. In like manner, and under the considerations in Art. 16, it is clear that the knowledge of Y on the whole earth, combined with the knowledge of X at all points of a line run-

ning from one pole of the earth to the other, is sufficient for the foundation of the *complete* theory of the magnetism of the earth.

IV. Finally, it is clear that the complete theory is also deducible from the simple knowledge of the value of Z on the whole surface of the earth. In fact, if Z be developed into a series,

$$Z = Q^0 + Q' + Q'' + Q''' +, \&c.$$

so that the general member satisfies the often-mentioned partial differential equation; Q^0 must necessarily $= 0$, and

$$P' = \tfrac{1}{2} Q', P'' = \tfrac{1}{3} Q'', P''' = \tfrac{1}{4} Q''', \&c.$$

21.

On account of the simple nature of the dependence of the several forces X, Y, Z, on a single function V, and the simple relation which they bear to each other, they are far better calculated to serve as a foundation for the theory, than the usual expression of the magnetic force by the three elements, total intensity, inclination, and declination. Or rather, the latter mode, natural as it appears in itself when the question is solely that of comprehending the facts, cannot directly serve for the foundation of the theory (at least not for the first foundation) until it has been translated into the other form.

In this view it would be very desirable that a general graphical representation of the horizontal intensity should be made; partly because it would be more immediately useful for theory than the total intensity; partly because, in far the greater number of cases, the horizontal intensity was originally that which was actually observed, the total intensity having been subsequently deduced from it by means of the dip. It is the more advisable to keep the elements of the horizontal force unmixed, as they can be determined with extreme accuracy with the present instrumental means; at any rate, the observed horizontal intensity should never be suppressed when publishing the deduced total intensity, without at least giving the dip employed in the calculation; so that a person wishing to employ the horizontal intensity for the theory may either have, or be enabled to reproduce, the original observed numbers.

Interesting as it would be to found the theory of terrestrial magnetism on observations of the horizontal needle only, and thus to anticipate the vertical part, or the inclination, it is at present

much too soon to do so: the scantiness of the data which we now possess does not allow of our dispensing with the assistance of the vertical part. It is a confirmation of the theory, if we can show the agreement of the different elements when reduced to one principle.

22.

Although we are *a priori* certain that the series for V, X, Y, Z, converge, nothing can be determined beforehand as to the degree of convergence. If the seats of the magnetic forces be limited to a moderate space around the centre of the earth, or if there were such a distribution of the magnetic fluids in the earth as to be equivalent thereto, the series would converge very rapidly; on the other hand, the further the seats of the magnetic forces extend towards the surface, and the more irregular the distribution, the slower we must be prepared to find the convergence.

In the practical application, absolute exactness is unattainable; we have to desire only a degree of approximation commensurate with the circumstances. The slower the convergence, the greater will be the number of members which must be taken into account to attain a certain degree of accuracy.

Now, P' contains three members, and requires, therefore, the knowledge of three co-efficients $g^{1.0}$, $g^{1.1}$, $h^{1.1}$; P'' requires five co-efficients; P''' seven; P^{iv} nine, &c. As we consider P', P'', P''', &c. as magnitudes of the first, second, and third order, and so on, it is clear that if the calculation is to be pushed to magnitudes of the order n inclusive, the values of $n^2 + 2\,n$ co-efficients must be determined; therefore, for example, 24 coefficients, if we would go as far as the fourth order.

Every given value of X, Y, or Z, for given values of u and λ, furnishes an equation between the co-efficients, whilst for each place where the complete elements of the terrestrial magnetic force are known, three equations are given. If we could venture to assume that the members have a sensible influence only as far as the fourth order, complete observations from eight points would be sufficient, theoretically considered, for the determination of all the co-efficients. But such a supposition can hardly be ventured upon, and the accidental errors which beset all observations, together with the neglected members of higher orders,

might have a very injurious effect on the results of the elimination*.

To diminish the unfavourable effect of these circumstances, the number of series of observations from stations well distributed over the whole globe ought to be much greater than that of the unknown values, and these should be derived from the observations by the method of least squares. As all the equations are only linear, the process would, it is true, be uniform; but the extent of the labour, arising from the great number of unknown values and equations, would be such as might well deter the most courageous calculator from undertaking it in this form, especially as the result might be wholly vitiated by the introduction either of defective observations or of accidental errors of calculation.

23.

There is another mode of proceeding, which, as it is free from a part of these difficulties, appears better adapted for a first trial. We shall develope it in this place without omitting to notice objections to which its application may be liable in the present state of the inquiry. This method supposes the knowledge of all three elements at points so grouped on a sufficient number of parallels as to divide them into a sufficient number of equal portions. The numerical values of X, Y, and Z, are to be first deduced from the given elements of the usual form.

The values of X, Y, Z, are then brought by the known method in each parallel to the form

$$X = k + k' \cos\lambda + K' \sin\lambda + k'' \cos 2\lambda + K'' \sin 2\lambda$$
$$+ k''' \cos 3\lambda + K''' \sin 3\lambda +, \&c.$$
$$Y = l + l' \cos\lambda + L' \sin\lambda + l'' \cos 2\lambda + L'' \sin 2\lambda$$
$$+ l''' \cos 3\lambda + L''' \sin 3\lambda +, \&c.$$
$$Z = m + m' \cos\lambda + M' \sin\lambda + m'' \cos 2\lambda + M'' \sin 2\lambda$$
$$+ m''' \cos 3\lambda + M''' \sin 3\lambda +, \&c.$$

We then obtain as many values for each of the co-efficients k, l, m, k', &c., as there are parallels of latitude under consideration.

Theory would give in each parallel $l = o$; therefore the values of l which result from the calculation furnish a kind of measure

* In such a mode of determination, the effect of these circumstances would be least injurious if the eight points were distributed symmetrically on the surface of the earth; that is to say, if they coincided, or nearly so, with the corners of a cube inscribed in the globe.

of the degree of uncertainty which still attaches to the fundamental members.

From the equations

$$k = -g^{1\cdot 0}\frac{d\,P^{1\cdot 0}}{d\,u} - g^{2\cdot 0}\frac{d\,P^{2\cdot 0}}{d\,u} - g^{3\cdot 0}\frac{d\,P^{3\cdot 0}}{d\,u} -, \text{\&c.}$$

$$m = 2\,g^{1\cdot 0}\,P^{1\cdot 0} + 3\,g^{2\cdot 0}\,P^{2\cdot 0} + 4\,g^{3\cdot 0}\,\rho^{3\cdot 0} +, \text{\&c.},$$

the total number of which is double the number of the parallels, we have to obtain, by the method of least squares, (after substituting in $\frac{d\,P^{1\cdot 0}}{d\,u}$, &c., and in $P^{1\cdot 0}$, &c. the corresponding numerical values of u,) as many of the co-efficients $g^{1\cdot 0}$, $g^{2\cdot 0}$, $g^{3\cdot 0}$, &c. as require to be taken into account.

In like manner the equations

$$-k' = g^{1\cdot 1}\frac{d\,P^{1\cdot 1}}{d\,u} + g^{2\cdot 1}\frac{d\,P^{2\cdot 1}}{d\,u} + g^{3\cdot 1}\frac{d\,P^{3\cdot 1}}{d\,u} +, \text{\&c.}$$

$$L' = g^{1\cdot 1}\frac{P^{1\cdot 1}}{\sin u} + g^{2\cdot 1}\frac{P^{2\cdot 1}}{\sin u} + g^{3\cdot 1}\frac{P^{3\cdot 1}}{\sin u} +, \text{\&c.}$$

$$m' = 2\,g^{1\cdot 1}\,P^{1\cdot 1} + 3\,g^{2\cdot 1}\,P^{2\cdot 1} + 4\,g^{3\cdot 1}\,P^{3\cdot 1} +, \text{\&c.},$$

the number of which is three times as great as the number of parallels, serve to determine the co-efficients $g^{1\cdot 1}$, $g^{2\cdot 1}$, $g^{3\cdot 1}$, &c. And the following,

$$-K' = h^{1\cdot 1}\frac{d\,P^{1\cdot 1}}{d\,u} + h^{2\cdot 1}\frac{d\,P^{2\cdot 1}}{d\,u} + h^{3\cdot 1}\frac{d\,P^{3\cdot 1}}{d\,u} +, \text{\&c.}$$

$$-l' = h^{1\cdot 1}\frac{P^{1\cdot 1}}{\sin u} + h^{2\cdot 1}\frac{P^{2.1}}{\sin u} + h^{3\cdot 1}\frac{P^{3\cdot 1}}{\sin u} +, \text{\&c.}$$

$$M' = 2\,h^{1.1}\,P^{1\cdot 1} + 3\,h^{2\cdot 1}\,P^{2\cdot 1} + 4\,h^{3\cdot 1}\,P^{3\cdot 1} +, \text{\&c.}$$

determine the coefficients $h^{1\cdot 1}$, $h^{2\cdot 1}$, $h^{3\cdot 1}$, &c. Further, the equations

$$-k'' = g^{2\cdot 2}\frac{d\,P^{2.2}}{d\,u} + g^{3\cdot 2}\frac{d\,P^{3\cdot 2}}{d\,u} + g^{4.2}\frac{d\,P^{4\cdot 2}}{d\,u} +, \text{\&c.}$$

$$L'' = 2\,g^{2\cdot 2}\frac{P^{2\cdot 2}}{\sin u} + 2\,g^{3\cdot 2}\frac{P^{3\cdot 2}}{\sin u} + 2\,g^{4\cdot 2}\frac{P^{4.2}}{\sin u} +, \text{\&c.}$$

$$m'' = 3\,g^{2\cdot 2}\,P^{2\cdot 2} + 4\,g^{3\cdot 2}\,P^{3\cdot 2} + 5\,g^{4\cdot 2}\,P^{4\cdot 2} +, \text{\&c.}$$

determine the co-efficients $g^{2\cdot 2}$, $g^{3\cdot 2}$, $g^{4\cdot 2}$, &c.; and we obtain the co-efficients of the succeeding higher numbers in a similar manner.

24.

The chief advantage which this method possesses over that

given in Art. 22, consists in the unknown values being broken into groups, each of which is determined by itself, whereby the calculation is greatly facilitated; whereas, in the other mode of proceeding, the intermingling of all the unknown quantities renders their separation extremely difficult. On the other hand, the disadvantage of the second method is, that, instead of being founded on direct observation, it rests on graphical representations, which, in districts where we do possess observations, represent them but rudely, and which, in districts where observations are wanting, are only conjectural, and, to a certain degree, arbitrary, and may therefore differ considerably from the truth. However, we must either postpone all attempts, till such time as we shall be provided with far more complete and trustworthy data than we now possess, or, with our present very scanty means, make a first attempt, from which we are entitled to expect little more than a rough approximation. A close comparison of the results of calculation with those of actual observation in all parts of the earth, furnishes a certain standard by which our success may be estimated. And if this test shall show that the first attempt has not entirely failed, it will powerfully assist suitable preparations for future fresh attempts by either method.

25.

Several years ago I repeatedly began attempts of this kind, from all of which the great inadequacy of the data at my command forced me to desist. I might earlier have concluded such an essay if I had obtained the fulfilment of my often-expressed wish for a general map representing the horizontal intensity. This want could not be supplied by the combination of the imperfect general maps of dip and of total intensity, then existing.

The appearance of Sabine's Map of the Total Intensity (in the *Seventh Report of the British Association for the Advancement of Science*) has stimulated me to undertake and complete a new attempt, which must be regarded, however, only in the light mentioned in the foregoing article. The data employed in the calculations are for twelve points on seven parallels. They are taken for the intensity from the above-mentioned map; for the declination from Barlow's map (*Phil. Trans.* 1833); and for the inclination from Horner's map (*Physikalisches Wörterbuch*, Band vi.). Considerable portions of these maps still remain blank,

and we can only fill these up with the greatest uncertainty. It was soon found that the calculation must be pushed at least as far as magnitudes of the fourth order, making the number of co-efficients to be determined amount to twenty-four. In all probability, members of the fifth order will also be found influential; but, in a first attempt, the values of k, m, k', &c. must be still too much charged with errors, arising from the uncertainty of many of the data (and which from their nature these values involve), to permit the introduction of a still greater number of unknown values in the process of elimination.

It should be remarked that the intensities in Sabine's map are expressed according to the arbitrary unity in common use, by which the total intensity in London = 1·372. In these calculations, and in the tables given in the sequel, this unity has been altered so as to make all the numbers a thousand times greater, the intensity in London on which they rest being made = 1372. It is obvious that a unity for the intensity may be taken at pleasure, since the unity for μ may be considered as arbitrary, and made to accord therewith. If further deductions are desired requiring μ to be reduced to absolute measure, it will only be necessary to multiply all the co-efficients by the factor which reduces to an absolute measure the intensities expressed according to the arbitrary unity.

26.

The numerical values of the 24 co-efficients obtained by the first calculation, the longitude λ being reckoned east from Greenwich, are as follows:

$g^{1\cdot0} = + 925\cdot782$	$g^{2\cdot2} = + 0\cdot493$
$g^{2\cdot0} = - 22\cdot059$	$g^{3\cdot2} = - 73\cdot193$
$g^{3\cdot0} = - 18\cdot868$	$g^{4\cdot2} = - 45\cdot791$
$g^{4\cdot0} = - 108\cdot855$	$h^{2\cdot2} = - 39\cdot010$
$g^{1\cdot1} = + 89\cdot024$	$h^{3\cdot2} = - 22\cdot766$
$g^{2\cdot1} = - 144\cdot913$	$h^{4\cdot2} = + 42\cdot573$
$g^{3\cdot1} = + 122\cdot936$	$g^{3\cdot3} = + 1\cdot396$
$g^{4\cdot1} = - 152\cdot589$	$g^{4\cdot3} = + 19\cdot774$
$h^{1\cdot1} = - 178\cdot744$	$h^{3\cdot3} = - 18\cdot750$
$h^{2\cdot1} = - 6\cdot030$	$h^{4\cdot3} = - 0\cdot178$
$h^{3\cdot1} = + 47\cdot794$	$g^{4\cdot4} = + 4\cdot127$
$h^{4\cdot1} = + 64\cdot112$	$h^{4\cdot4} = + 3\cdot175$

These numbers, which may be considered as the *elements of*

the theory of terrestrial magnetism, are used both here and in the formation of the table to be described in the sequel, just as they were given by calculation, without omitting decimals. To any one conversant with calculation it is superfluous to remark, that these fractional parts have in themselves no value, as we are still far from being able to eliminate with certainty even the integers. But it is important that the observations should be closely compared with one and the same definite system of elements; and, as by leaving out decimals nothing would be gained in point of convenience in computing, there was no reason for altering in any respect the elements given by calculation.

27.

The expression for V, developed according to the above numbers, is as follows: for the sake of brevity e stands for cos u, and f for sin u.

$$
\begin{aligned}
\frac{V}{R} = & -1{\cdot}977 + 937{\cdot}103\,e + 71{\cdot}245\,e^2 - 18{\cdot}868\,e^3 \\
& \qquad - 108{\cdot}855\,e^4 \\
& + (64{\cdot}437 - 79{\cdot}518\,e + 122{\cdot}936\,e^2 + 152{\cdot}589\,e^3)\,f\cos\lambda \\
& + (-188{\cdot}303 - 33{\cdot}507\,e + 47{\cdot}794\,e^2 + 64{\cdot}112\,e^3)\,f\sin\lambda \\
& + (7{\cdot}035 - 73{\cdot}193\,e - 45{\cdot}791\,e^2)\,f^2\cos 2\lambda \\
& + (-45{\cdot}092 - 22{\cdot}766\,e - 42{\cdot}573\,e^2)\,f^2\sin 2\lambda \\
& + (1{\cdot}396 + 19{\cdot}774\,e)\,f^3\cos 3\lambda \\
& + (-18{\cdot}750 - 0{\cdot}178\,e)\,f^3\sin 3\lambda \\
& + 4{\cdot}127\,f^4\cos 4\lambda \\
& + 3{\cdot}175\,f^4\sin 4\lambda.
\end{aligned}
$$

We may here add the completely developed expressions for the three components of the magnetic force.

$$
\begin{aligned}
X = & (937{\cdot}103 + 142{\cdot}490\,e - 56{\cdot}603\,e^2 - 435{\cdot}420\,e^3)\,f \\
& + (-79{\cdot}518 + 181{\cdot}435\,e - 298{\cdot}732\,e^2 - 368{\cdot}808\,e^3 \\
& \qquad + 610{\cdot}357\,e^4)\cos\lambda \\
& + (-33{\cdot}507 + 283{\cdot}892\,e + 259{\cdot}349\,e^2 - 143{\cdot}383\,e^3 \\
& \qquad - 256{\cdot}448\,e^4)\sin\lambda \\
& + (-73{\cdot}193 - 105{\cdot}652\,e + 219{\cdot}579\,e^2 + 183{\cdot}164\,e^3)\,f\cos 2\lambda \\
& + (-22{\cdot}766 + 175{\cdot}330\,e + 68{\cdot}098\,e^2 - 170{\cdot}292\,e^3)\,f\sin 2\lambda \\
& + (19{\cdot}774 - 4{\cdot}188\,e - 79{\cdot}096\,e^2)\,f^2\cos 3\lambda \\
& + (-0{\cdot}178 + 56{\cdot}250\,e + 0{\cdot}716\,e^2)\,f^2\sin 3\lambda \\
& - 16{\cdot}508\,ef^3\cos 4\lambda \\
& - 12{\cdot}701\,ef^3\sin 4\lambda \\
Y = & (188{\cdot}303 + 33{\cdot}507\,e - 47{\cdot}794\,e^2 - 64{\cdot}112\,e^3)\cos\lambda
\end{aligned}
$$

$$+ (64{\cdot}437 - 79{\cdot}518\, e + 122{\cdot}936\, e^2 - 152{\cdot}589\, e^3) \sin \lambda$$
$$+ (90{\cdot}184 + 45{\cdot}532\, e - 185{\cdot}46\, e^2)\, f \cos 2\,\lambda$$
$$+ (14{\cdot}070 - 146{\cdot}386\, e - 91{\cdot}582\, e^2)\, f \sin 2\,\lambda$$
$$+ (56{\cdot}250 + 0{\cdot}534\, e)\, f^2 \cos 3\,\lambda$$
$$+ (4{\cdot}188 + 59{\cdot}322\, e)\, f^2 \sin 3\,\lambda$$
$$- 12{\cdot}701\, f^3 \cos 4\,\lambda$$
$$+ 16{\cdot}508\, f^3 \sin 4\,\lambda$$
$$Z = - 24{\cdot}593 + 1896.847\, e + 400{\cdot}343\, e^2 - 75{\cdot}471\, e^3 - 544{\cdot}275\, e^4$$
$$+ (79{\cdot}700 - 107{\cdot}763\, e + 491{\cdot}744\, e^2 - 762{\cdot}946\, e^3)\, f \cos \lambda$$
$$+ (-395{\cdot}724 - 155{\cdot}473\, e + 191{\cdot}176\, e^2 + 320{\cdot}560\, e^3)\, f \sin \lambda$$
$$+ (34{\cdot}187 - 292{\cdot}772\, e - 228{\cdot}955\, e^2)\, f^2 \cos 2\,\lambda$$
$$+ (-147{\cdot}439 - 91{\cdot}064\, e + 212{\cdot}865\, e^2)\, f^2 \sin 2\,\lambda$$
$$+ (5{\cdot}584 + 98{\cdot}870\, e)\, f^3 \cos 3\,\lambda$$
$$+ (-75{\cdot}000 - 0{\cdot}890\, e)\, f^3 \sin 3\,\lambda$$
$$+ 20{\cdot}635\, f^4 \cos 4\,\lambda$$
$$+ 15{\cdot}876\, f^4 \sin 4\,\lambda.$$

After these components have been calculated for a given place, we obtain in the following manner the several parts of the determination of the magnetic force, according to the customary form.

Let δ be the declination, i the inclination, ψ the total, and ω the horizontal intensity. Determine first δ and ω by means of the formulæ

$$X = \omega \cos \delta,\ Y = \omega \sin \delta,$$

and then i and ψ by means of the following formulæ:

$$\omega = \psi \cos i,\ Z = \psi \sin i.$$

28.

As the formulæ for X, Y, Z, contain 71 members, their immediate calculation is a considerable labour. Its repetition for a great number of places appears the more alarming, considering that we could hardly hope to be secure from the possibility of mistake without going twice over the whole. But little would be gained by suppressing all those members of which the co-efficients are less than an integer, or even less than 10 integers, for the remaining members would still amount to 65. But as the whole value of the work would remain uncertain if not tested by a considerable number of actual observations, I have encountered the labour of calculating a table, by the assistance of which the work will be in the highest degree

abridged and facilitated, and at the same time the important object of security against errors of calculation will be materially promoted.

For the construction of the table the values of the coefficients were brought into the following form:

$$X = a^0 + a' \cos(\lambda + A') + a'' \cos(2\lambda + A'') + a''' \cos(3\lambda + A''') + a^{IV} \cos(4\lambda + A^{IV})$$

$$Y = b' \cos(\lambda + B') + b'' \cos(2\lambda + B'') + b''' \cos(3\lambda + B''') + b^{IV} \cos(4\lambda + B^{IV})$$

$$Z = c^0 + c' \cos(\lambda + C') + c'' \cos(2\lambda + C'') + c''' \cos(3\lambda + C''') + c^{IV} \cos(4\lambda + C^{IV}).$$

The first table contains those parts of X and Z which are independent of λ. In the four next tables are found the values of the auxiliary angles A', A'', &c., and the logarithms of a', a'', &c., all for the several degrees of latitude $\phi = 90° - u$. The table is placed at the end of the memoir.

The calculation for Göttingen is given as an example.

For latitude 51° 32′ we find from the tables:

$a^0 = +500{\cdot}8$		$c^0 = +1465{\cdot}2$
$\log a' = 2{\cdot}28980$	$\log b' = 2{\cdot}18900$	$\log c' = 2{\cdot}20204$
$\log a'' = 1{\cdot}79403$	$\log b'' = 2{\cdot}03220$	$\log c'' = 2{\cdot}12777$
$\log a''' = 1{\cdot}32522$	$\log b''' = 1{\cdot}46845$	$\log c''' = 1{\cdot}43199$
$\log a^{IV} = 0{\cdot}59391$	$\log b^{IV} = 0{\cdot}70016$	$\log c^{IV} = 0{\cdot}59091$
$A' = 249°\ 30'$	$B' = 358°\ 24'$	$C' = 105°\ 44'$
$A'' = 311\ \ 45$	$B'' = 64\ \ 50$	$C'' = 165\ \ 15$
$A''' = 234\ \ 10$	$B''' = 318\ \ 13$	$C''' = 42\ \ 22$
$A^{IV} = 142\ \ 26$	$B^{IV} = 232\ \ 26$	$C^{IV} = 322\ \ 26$

And for longitude 9° 56′½, the parts of X, Y, Z, are found as follows:

X	Y	Z
+ 500·8		+ 1465·2
− 35·71	+ 152·89	− 68·99
+ 54·76	+ 9·92	− 133·67
− 2·21	+ 28·77	+ 8·27
− 3·92	+ 0·19	+ 3·90
$X = +513{\cdot}72$	$Y = +191{\cdot}77$	$Z = +1274{\cdot}71$

The farther calculation then gives :

$$\delta = +\ 20^\circ\ 28' \quad \log \omega = 2{\cdot}73907.$$
$$i = +\ 66\ \ 43$$
$\psi = 1387{\cdot}6$, or, in the unity commonly employed,
$$\psi = 1{\cdot}3876.$$

29.

The following table contains the comparison of our formulæ, with observations at 91 stations in all parts of the earth. As the three maps from which we have taken the data for our calculation are intended to represent the phænomena for the most recent epoch, we have included in our comparison only very recent observations, and we have taken, by preference, observations at those stations where all the three magnetic elements were observed. We are not at present in a condition to demand that the observations should be strictly cotemporaneous, unless we would see our stock reduced to a very small number.

		Latitude.	Longitude.	Declination. Computed.	Declination. Observed.	Declination. Difference.
		° ′	° ′	° ′	° ′	° ′
1	Spitzbergen	+79 50	11 40	+26 31	+25 12	+ 1 19
2	Hammerfest........	70 40	23 46	+12 23	+10 50	+ 1 33
3	Mag. Pole of Ross.	70 5	263 14	−22 23		
4	Reikiavik	64 8	338 5	+40 12	+43 14	− 3 2
5	Jakutsk............	62 1	129 45	+ 0 5	+ 5 50	− 5 45
6	Porotowsk	62 1	131 50	+ 0 4	+ 4 46	− 4 42
7	Nochinsk	61 57	134 57	− 0 3	+ 2 11	− 2 14
8	Tschernoljes	61 31	136 23	0 0	+ 3 30	− 3 30
9	Petersburg	59 56	30 19	+ 6 47	+ 6 44	+ 0 3
10	Christiania	59 54	10 44	+19 55	+19 50	+ 0 5
11	Ochotsk	59 21	143 11	− 0 18	+ 2 18	2 36
12	Tobolsk	58 11	68 16	− 7 19	−10 29	+ 3 10
13	Tigil River	58 1	158 15	− 4 20	− 4 6	− 0 14
14	Sitka..............	57 3	224 35	−28 45	−28 19	− 0 26
15	Tara	56 54	74 4	− 7 44	− 9 36	+ 1 52
16	Catharinenburg	56 51	60 34	− 5 20	− 6 18	+ 0 58
17	Tomsk	56 30	85 9	− 7 21	− 8 34	+ 1 13
18	Nishny Novogorod..	56 19	43 57	+ 1 10	− 0 27	+ 1 37
19	Krasnojarsk	56 1	92 57	− 5 49	− 6 40	+ 0 51
20	Kasan	55 48	49 7	− 1 7	− 2 22	+ 1 15
21	Moscow	55 46	37 37	+ 4 26	+ 3 2	+ 1 24
22	Königsberg	54 43	20 30	+14 15	+13 22	+ 0 53
23	Barnaul	53 20	83 56	− 7 0	− 7 25	+ 0 25
24	Uststretensk	53 20	121 51	+ 1 29	+ 4 21	− 2 52
25	Gorbizkoi..........	53 6	119 9	+ 1 5	+ 2 54	− 1 49
26	Petropaulowsk	53 0	158 40	− 3 34	− 4 6	+ 0 32
27	Uriupina	52 47	120 4	+ 1 16	+ 4 4	− 2 48
28	Berlin	52 30	13 24	+18 31	+17 5	+ 1 26
29	Pogromnoi	52 30	111 3	− 0 38	+ 0 18	− 0 56
30	Irkutsk	52 17	104 17	− 2 27	− 1 38	− 0 49
31	Stretensk	52 15	117 40	+ 0 54	+ 2 52	− 1 58
32	Stepnoi............	52 10	106 21	− 1 52	− 1 8	− 0 44
33	Tschitanskoi	52 1	113 27	0 0	+ 1 13	− 1 13
34	Nertschinsk	51 56	116 31	+ 0 42	+ 2 53	− 2 11
35	Werchneudinsk	51 50	107 46	− 1 26	− 0 24	− 1 2
36	Orenburg..........	51 45	55 6	− 2 48	− 3 22	+ 0 34
37	Argunskoi	51 33	119 56	+ 1 22	+ 3 44	− 2 22
38	Göttingen..........	51 32	9 56	+20 28	+18 38	+ 1 50
39	London............	51 31	359 50	+25 37	+24 0	+ 1 37
40	Nertschinsk Mine ..	51 19	119 37	+ 1 20	+ 4 6	− 2 46
41	Tschindant	50 34	115 32	+ 0 34	+ 2 14	− 1 40
42	Charazaiska........	50 29	104 44	− 2 9	− 2 27	+ 0 18
43	Zuruchaitu	50 23	119 3	+ 1 18	+ 3 11	− 1 53
44	Troizkosawsk	50 21	106 45	− 1 34	− 0 12	− 1 22
45	Abagaitujewskoi....	49 35	117 50	+ 1 8	+ 2 54	− 1 46
46	Altanskoi	49 28	111 30	− 0 16	+ 0 48	− 1 4
47	Mendschinskoi	49 26	108 55	− 0 56	+ 0 12	− 1 8
48	Paris	48 52	2 21	+24 6	+22 4	+ 2 2
49	Chunzal	48 13	106 27	− 1 30	− 1 6	− 0 24
50	Urga	47 55	106 42	− 1 26	− 1 16	− 0 10

	Inclination.			Intensity.		
	Computed.	Observed.	Difference.	Computed.	Observed.	Difference.
	° ′	° ′	° ′			
1	+ 82 1	+ 81 11	+ 0 50	1·599	1·562	+ 0·037
2	77 19	77 15	+ 0 4	1·545	1·506	+ 0·039
3	88 48	90 0	− 1 12	1·717		
4	80 40	77 0	+ 3 40	1·527		
5	74 36	74 18	+ 0 18	1·661	1·697	− 0·036
6	74 27	74 0	+ 0 27	1·658	1·721	− 0·063
7	74 12	73 37	+ 0 35	1·653	1·713	− 0·060
8	73 48	73 8	+ 0 40	1·648	1·700	− 0·052
9	70 25	71 3	− 0 38	1·469	1·410	+ 0·059
10	72 4	72 7	− 0 3	1·456	1·419	+ 0·037
11	71 36	70 41	+ 0 55	1·621	1·615	+ 0·006
12	70 13	71 1	− 0 48	1·575	1·557	+ 0·018
13	69 55	68 28	+ 1 27	1·583	1·577	+ 0·006
14	76 30	75 51	+ 0 39	1·697	1·731	− 0·034
15	69 46	70 28	− 0 42	1·586	1·575	+ 0·011
16	68 24	69 16	− 0 52	1·535	1·523	+ 0·012
17	70 33	70 55	− 0 22	1·613	1·619	− 0·006
18	67 9	68 41	− 1 32	1·469	1·442	+ 0·027
19	70 24	71 0	− 0 36	1·638	1·657	− 0·019
20	67 13	68 25	− 1 12	1·477	1·433	+ 0·044
21	66 45	68 57	− 2 12	1·446	1·404	+ 0·042
22	67 19	69 26	− 2 7	1·410	1·365	+ 0·045
23	67 50	68 10	− 0 20	1·591	1·605	− 0·014
24	68 32	68 11	+ 0 21	1·609	1·656	− 0·047
25	68 32	68 22	+ 0 10	1·611	1·660	− 0·049
26	65 31	63 50	+ 1 41	1·521	1·489	+ 0·032
27	68 17	67 53	+ 0 24	1·612	1·667	− 0·055
28	66 45	68 7	− 1 22	1·391	1·367	+ 0·024
29	68 25	68 8	+ 0 17	1·616	1·640	− 0·024
30	68 17	68 14	+ 0 3	1·616	1.647	− 0·031
31	67 55	67 38	+ 0 17	1·606	1·649	− 0·043
32	68 12	68 10	+ 0 2	1·615	1·663	− 0·048
33	67 56	67 42	+ 0 14	1·609	1·668	− 0·059
34	67 43	67 11	+ 0 32	1·604	1·635	− 0·031
35	67 55	68 6	− 0 11	1·612	1·657	− 0·045
36	63 14	64 44	− 1 30	1·461	1·432	+ 0·029
37	67 10	66 54	+ 0 16	1·595	1·655	− 0·060
38	66 43	67 56	− 1 13	1·388	1·357	+ 0·031
39	68 54	69 17	− 0 23	1·410	1·372	+ 0·038
40	66 59	66 33	+ 0 26	1·593	1·617	− 0·024
41	66 35	66 32	+ 0 3	1·592	1·650	− 0·058
42	66 45	66 56	− 0 11	1·599	1·643	− 0·044
43	66 12	66 13	− 0 1	1·584	1·626	− 0·042
44	66 38	66 19	+ 0 19	1·597	1·642	− 0·045
45	65 33	64 48	+ 0 45	1·577	1·583	− 0·006
46	65 46	65 20	+ 0 26	1·585	1·619	− 0·034
47	65 48	65 31	+ 0 17	1·587	1·630	− 0·043
48	66 45	67 24	− 0 39	1·389	1·348	+ 0·041
49	64 42	64 29	+ 0 13	1·574	1·612	− 0·038
50	64 25	64 4	+ 0 21	1·571	1·583	− 0·012

		Latitude.	Longitude.	Declination. Computed.	Declination. Observed.	Declination. Difference.
		° ′	° ′	° ′	° ′	° ′
51	Astrachan..........	+46 20	48 0	+ 1 40	+ 1 12	+ 0 28
52	Cholugur	46 0	110 34	− 0 20	+ 0 49	− 1 9
53	Ergi	45 32	111 25	− 0 6	+ 1 7	− 1 13
54	Milan	45 28	9 9	+20 56	+18 33	+ 2 23
55	Sendschi	44 45	110 26	− 0 20	+ 0 30	− 0 50
56	Batchay	44 21	112 55	+ 0 16	+ 0 59	− 0 43
57	Scharabudurguna ..	43 13	114 6	+ 0 32	+ 0 46	− 0 14
58	Naples	40 52	14 6	+18 53	+15 20	+ 3 33
59	Chalgan	40 49	114 58	+ 0 42	+ 1 13	− 0 31
60	Pekin	39 54	116 26	+ 0 58	+ 1 48	− 0 50
61	Terceira	38 39	332 47	+25 17	+24 18	+ 0 59
62	San Francisco......	37 49	237 35	−16 22	−14 55	− 1 27
63	Port Praya	14 54	336 30	+16 17	+16 30	− 0 13
64	Madras............	13 4	80 17	− 4 1		
65	Galapagos Island ..	− 0 50	270 23	− 8 57	− 9 30	+ 0 33
66	Ascension	7 56	345 36	+14 37	+13 30	+ 1 7
67	Pernambuco	8 4	325 9	+ 5 58	+ 5 54	+ 0 4
68	Callao	12 4	285 46	− 9 6	−10 0	+ 0 54
69	Keeling's Islands....	12 5	96 55	+ 0 23	+ 1 12	− 0 49
70	Bahia	12 59	321 30	+ 3 12	+ 4 18	− 1 6
71	St. Helena	15 55	354 17	+18 48	+18 0	+ 0 48
72	Otaheite	17 29	210 30	− 5 45	− 7 34	+ 1 49
73	Mauritius..........	20 9	57 31	+11 9	+11 18	− 0 9
74	Rio de Janeiro	22 55	316 51	− 1 11	− 2 8	+ 0 57
75	Valparaiso	33 2	288 19	−13 45	−15 18	+ 1 33
76	Sydney	33 51	151 17	− 7 51	−10 24	+ 2 33
77	Cape of Good Hope	34 11	18 26	+27 24	+28 30	− 1 6
78	Monte Vid	34 53	303 47	−11 23	−12 0	+ 0 37
79	K. George Sound ..	35 2	117 56	+ 5 12	+ 5 36	− 0 24
80	New Zealand	35 16	174 0	−11 10	−14 0	+ 2 50
81	Concepcion	36 42	286 50	−14 43	−16 48	+ 2 5
82	Blanco Bay........	38 57	298 1	−12 57	−15 0	+ 2 3
83	Valdivia	39 53	286 31	−16 13	−17 30	+ 1 17
84	Chiloe	41 51	286 4	−16 56	−18 0	+ 1 4
85	Hobarttown........	42 53	147 24	− 5 51	−11 6	+ 5 15
86	Port Low..........	43 48	285 58	−17 32	−19 48	+ 2 16
87	Port San Andres ..	46 35	284 25	−19 4	−20 48	+ 1 44
88	Port Desire........	47 45	294 5	−16 52	−20 12	+ 3 20
89	R. Santa Cruz......	50 7	291 36	−18 23	−20 54	+ 2 31
90	Falkland Islands....	51 32	301 53	−15 16	−19 0	+ 3 44
91	Port Famine	53 38	289 2	−20 28	−23 0	+ 2 32

	Inclination.			Intensity.		
	Computed.	Observed.	Difference.	Computed.	Observed.	Difference.
	° ′	° ′	° ′			
51	+ 56 59	+ 59 58	− 2 59	1·358	1·334	+ 0·024
52	62 31	61 54	+ 0 37	1·545	1·580	− 0·035
53	61 58	61 22	+ 0 36	1·539	1·559	− 0·020
54	62 13	63 48	− 1 35	1·331	1·294	+ 0·037
55	61 15	60 42	+ 0 33	1·529	1·530	− 0·001
56	60 46	60 18	+ 0 28	1·520	1·553	− 0·033
57	59 32	59 3	+ 0 29	1·502	1·538	− 0·036
58	56 26	58 53	− 2 27	1·271	1·271	0·
59	56 51	56 17	+ 0 34	1·465	1·459	+ 0·006
60	55 43	54 49	+ 0 54	1·448	1·453	− 0·005
61	68 34	68 6	+ 0 28	1·469	1·457	+ 0·012
62	64 14	62 38	+ 1 36	1·592	1·591	+ 0·001
63	45 51	46 3	− 0 12	1·168	1·156	+ 0·012
64	4 14	6 52	− 2 38	1·038	1·031	+ 0·007
65	13 24	9 29	+ 3 55	1·085	1·069	+ 0·016
66	5 32	1 39	+ 3 53	0·813	0·873	− 0·060
67	13 2	13 13	− 0 11	0·909	0·914	− 0·005
68	− 3 23	− 7 3	+ 3 40	0·994		
69	− 39 19	− 38 33	− 0 46	1·161		
70	+ 3 59	+ 5 24	− 1 25	0·883	0·871	+ 0·012
71	− 14 55	− 18 1	+ 3 6	0·808	0·836	− 0·028
72	− 27 26	− 30 26	+ 3 0	1·113	1·094	+ 0·019
73	− 54 8	− 54 1	− 0 7	1·060	1·144	− 0·084
74	− 14 49	− 13 30	− 1 19	0·879	0·878	+ 0·001
75	− 37 56	− 39 7	+ 1 11	1·094	1·176	− 0·082
76	− 58 11	− 62 49	+ 4 38	1·667	1·685	− 0·018
77	− 51 4	− 52 35	+ 1 31	0·981	1·014	− 0·033
78	− 35 34	− 35 40	+ 0 6	1·022	1·060	− 0·038
79	− 62 39	− 64 41	+ 2 2	1·658	1·709	− 0·051
80	− 54 46	− 59 32	+ 4 46	1·616	1·591	+ 0·025
81	− 42 49	− 44 13	+ 1 24	1·147	1·218	− 0·071
82	− 42 1	− 41 54	− 0 7	1·103	1·113	− 0·010
83	− 46 13	− 46 47	+ 0 34	1·145	1·238	− 0·093
84	− 48 14	− 49 26	+ 1 12	1·227	1·313	− 0·086
85	− 66 57	− 70 35	+ 3 38	1·894	1·817	+ 0·077
86	− 50 4	− 51 20	+ 1 16	1·257	1·326	− 0·069
87	− 53 0	− 54 14	+ 1 14	1·310		
88	− 51 22	− 52 43	+ 1 21	1·263	1·359	− 0·096
89	− 53 49	− 55 16	+ 1 27	1·321	1·425	− 0·104
90	− 52 46	− 53 25	+ 0 39	1·276	1·367	− 0·091
91	− 57 38	− 59 53	+ 2 15	1·424	1·532	− 0·108

I add the following notices concerning the observations used in this comparison.

The determinations of the intensity are taken for the most part from Sabine's *report on the Variations of the Magnetic Intensity*

in the above-mentioned *Seventh Report of the British Association for the Advancement of Science.*

We are indebted for the great number of magnetic observations in the Russian Empire, and in the neighbouring parts of China, to

Hansteen. (Poggendorff's Annals.)

Erman. (*Reise um die Erde*, and manuscript communications.)

Von Humboldt. (*Voyage aux régions équinoxiales*, T. 13.)

Fuss. (*Mémoires de l'Académie des Sciences de St. Petersbourg, Sixième Série.*)

Fedor. (Communicated in manuscript, through Struve).

Reinke. (*Observations Météorologiques et Magnétiques, faites dans l'étendue de l'Empire de Russie, redigées par* A. T. Kupffer, Nr. II.)

At the following places a mean has been taken of the determinations of several observers. The differences between them are sometimes greater than can be attributed to yearly changes.

12. *Tobolsk.*

Declination.	Hansteen, 1828, . . .	− 9°	58′
	Erman, 1828, ...	− 9	47
	Fuss, 1830, ...	− 11	52
	Fedor, 1833, ...	− 10	20
Inclination.	Erman, 1828, ...	71	7
	Von Humboldt, 1829, . .	70	56
	Fuss, 1830, ...	71	1
	Fedor, 1833, ...	71	2

16. *Catharinenburg.*

Declination.	Hansteen, 1828, . . .	− 6°	27′
	Erman, 1828, ...	− 7	23
	Reinke, 1836, ...	− 5	5
Inclination.	Erman, 1828, ...	69	24
	Von Humboldt, 1829, . .	69	6
	Fuss, 1830, ...	69	19
	Fedor, 1832, ...	69	15

17. *Tomsk.*

Declination.	Hansteen, 1828, . . .	− 8°	32′
	Erman, 1829, ...	− 8	36
Inclination.	Erman, 1829, ...	70	59
	Fuss, 1830, ...	70	51

18. *Nishny Novogorod.*

Declination.	Erman, 1828, ...	− 0° 46′
	Fuss, 1830, ...	− 0 8

19. *Krasnojarsk.*

Declination.	Hansteen, 1829, ...	− 6° 43′
	Erman, 1829, ...	− 6 37
	Fedor, 1835, ...	− 7 26
Inclination.	Erman, 1829, ...	70 53
	Fedor, 1835, ...	71 8

20. *Kasan.*

Inclination.	Erman, 1828, ...	68° 21′
	Von Humboldt, 1829, . .	68 27
	Fuss, 1830, ...	68 26

21. *Moscow.*

Declination.	Hansteen, 1828, ...	+ 3° 3′
	Erman, 1828, ...	+ 3 1
Inclination.	Erman, 1828, ...	68 58
	Von Humboldt, 1829, . .	68 57

30. *Irkutsk.*

Declination.	Hansteen, 1829, ...	− 1° 37′
	Erman, 1829, ...	− 1 52
	Fuss, 1830, ...	− 1 25
Inclination.	Erman, 1829, ...	68 7
	Fuss, 1830, ...	68 15
	Fuss, 1832, ...	68 20

36. *Orenburg.*

Inclination.	Von Humboldt, 1829, . .	64° 41′
	Fedor, 1832, ...	64 47

44. *Troizkosawsk.*

Declination.	Hansteen, 1829, ...	+ 0° 5′
	Erman, 1829, ...	+ 0 33
	Fuss, 1830, ...	− 0 1
Inclination.	Erman, 1829, ...	66 14
	Fuss, 1830, ...	66 24

Most of the determinations in the southern hemisphere are

supplied by Captains King and Fitz Roy, and are taken from a little work by Sabine, (*Magnetic Observations made during the Voyages of H. B. M.'s Ships Adventure and Beagle*, 1826–1836.)

The determinations for the several other stations are taken partly from the above-named sources, and partly from the following:

1. Spitzbergen. Observer, Sabine, 1823. (*From his Account of Experiments to determine the Figure of the Earth.*)

2. Hammerfest. The declination and inclination are the means of the determinations of Sabine, 1823 (*Pendulum Experiments*); and of Parry, 1827. (*Narrative of an Attempt to reach the North Pole.*)

3. Magnetic Pole, from Captain James Ross, 1831. (*Phil. Trans.* 1834.)

4. Reikiavik, from observations by Lottin, 1836, (*Voyage en Islande.*)

28. Berlin, from Encke, 1836. (*Astronomisches Jahrbuch*, 1839.)

38. Göttingen. The declination is for October 1, 1835 (*Resultate für* 1836, page 39); the inclination is reduced to the same epoch by interpolation between von Humboldt's observation in 1826, and Forbes' in 1837.

39. London, from observations communicated in manuscript. The declination, by Captain James Ross, for the mean epoch, April, 1838; and the inclination by Phillips, Fox, Ross, Johnson, and Sabine, for the mean epoch of May, 1838.

48. Paris, for 1835, from the *Annuaire* for 1836.

54. Milan, 1837, by Kreil. Communicated by him in manuscript.

58. Naples, from observations by Sartorius and Listing. The intensity, which was determined according to absolute measure, has been reduced to the common unity, by the application of the factor given in Article 31.

64. Madras, 1837, from observations by Taylor, taken from the *Journal of the Asiatic Society of Bengal*, May, 1837.

30.

In judging of the differences between calculation and observation, as shown in the foregoing tabular comparison, it must be remembered, on the one hand, that almost all the observations are charged both with the errors of observation, and with the influ-

ence of the accidental anomalies of the magnetic force itself, and that they do not correspond to the same year*; and, on the other hand, that our formulæ do not include members beyond the fourth order, whereas those of the following order may still be very sensible. When due weight is allowed to these circumstances, the agreement between calculation and experiment appears to be as satisfactory as we are entitled to expect from a first attempt.

As our expression for $\frac{V}{R}$ may therefore be safely regarded as coming near the truth, at least in its more important members, it has appeared worth while to form a graphical representation of the course of the numerical values of this function. This has been done in a map drawn by Dr. Goldschmidt, in three parts, the first on Mercator's projection, passing round the globe, and including all the parallels between 70° north, and 70° south lat.; the other two being polar projections, extending to lat. 65°. The corrections and additions which will arise from a fresh calculation resting on more perfect data, may, doubtless, cause material alterations of position in these lines, particularly in the high southern latitudes; but no important change in the whole form of the system of lines can be supposed without such alterations in the expression for $\frac{V}{R}$ as would destroy the agreement with existing observations. We are thus led to the important result, that the system of lines of equal values of V, on the surface of the earth, is actually comprehended by the simplest type described in Art. 13, and that consequently there are on the earth *only two magnetic poles*, apart from the possible case of local exception spoken of in Art. 13.

* The last article presents instances of discordances between different observers at one and the same place; I will notice some others, which are much greater than can with any degree of probability be attributed to yearly changes. The dip at Valparaiso was, in 1829, according to King, 40° 11′; in 1835, according to Fitz Roy, 38° 3′. In Mauritius the intensity was 1·096 in 1818, according to Freycinet, and 1·192 in 1836, according to Fitz Roy. The difference is still greater at Otaheite, where Erman's intensity = 1·172 in 1830, and Fitz Roy's, in 1835, = 1·017. Otaheite is a station of the highest importance for the future improvement of the elements: the difference between the two determinations made there by different observers, considerably exceeds the greatest difference between the computed and observed intensities in our eighty-six comparisons.

The exact computation of the places of these two poles, according to our elements, gives them as follows:

1. In 73° 35′ north lat., 264° 21′ long. east from Greenwich, the value of the total intensity being = 1·701 in the unity in common use.

2. In 72° 35′ south lat., 152° 30′ long., the total intensity = 2·253.

At the first of these points $\frac{V}{R}$ has its greatest value, = + 895·86; at the second its smallest value, = − 1030·24.

According to Captain James Ross's observation the north magnetic pole falls 3° 30′ to the south of its position according to our calculation, which gives at that place a direction of the magnetic force, differing 1° 12′ from observation, as may be seen in the table of comparisons. We must expect a considerably greater displacement of the position of the southern pole. At Hobart Town, which is the nearest station to this pole, calculation gives too low a dip by 3° 38′, as far as the observation can be depended upon. It seems probable, therefore, that the actual south magnetic pole is considerably north of the position given by our calculation, and that it may be looked for in about 66° lat., and 146° long.

31.

The two points on the earth's surface where the horizontal force vanishes, and which are called magnetic poles, may, it is true, be allowed a certain significancy on account of their relation to the form of the phenomena of the horizontal force all over the earth; but we must be careful not to give them undue consideration. The chord which unites these two points has no significancy, and it would be a gross mistake to call it the *magnetic axis* of the earth. The only mode of giving a generally valid signification to the idea of the magnetic axis of a body is laid down in the 5th Article of the *Intensitas Vis Magneticæ*, where it is understood to mean the straight line in which the moment of the free magnetism contained in the body is a maximum. In order to determine both the position of the magnetic axis of the earth in this sense, and the moment of the earth's magnetism in relation to this same axis, we only require, as noticed in Art. 17, a knowledge of the members of the first order of V. According to our elements, Art. 26, $P' = + 925{\cdot}782 \cos u + 89{\cdot}024 \sin u$

$\cos\lambda - 178{\cdot}744 \sin u \sin\lambda$, and $-925{\cdot}782\ R^3$, $-89{\cdot}024\ R^3$, $+178{\cdot}744\ R^3$ are the moments of terrestrial magnetism with respect to the axis of the earth, and to the two radii for longitudes 0 and 90. In speaking of the earth's axis, the direction towards the north pole is to be understood, and the negative sign of the corresponding moment shows that the magnetic axis makes with it an obtuse angle, or that its magnetic north pole is turned towards the south.

The direction hence found for the magnetic axis is parallel to that diameter of the earth which is from 77° 50′ north lat., and 296° 29′ lon., to 77° 50′ south lat., 116° 29′ lon.; and the magnetic moment in relation to this axis is $= 947{\cdot}08\ R^3$. It must be remembered that in our elements the unity of intensity employed is a thousandth part of the unity in common use. In order to obtain the reduction to the absolute unity established in the *Intensitas Vis Magneticæ*, we must remark that in that work the horizontal intensity at Göttingen for the 19th of July, 1834, was found = 1·7748, which, combined with the dip 68° 1′, gives the total intensity = 4·7414. The total intensity, according to the unity employed above, was 1357. Thus the reducing factor is = 0·0034941, and the magnetic moment of the earth, expressed according to the absolute unity,

$$= 3{\cdot}3092\ R^3.$$

As the millimetre is the unit of length employed in the above absolute unity for the earth's magnetic force, R must also be given in millimetres; and, as the ellipticity of the earth need not be taken into account, it will be sufficient to consider R as the radius of a circle 40000 millions of millimetres in circumference. Hence the above magnetic moment will be expressed by a number of which the logarithm = 29,93136, or by 853800 quadrillions. By experiments made in the year 1832 (*Intensitas*, Art. 21) the magnetic moment of a magnet bar, of a pound weight, was found to be, according to the same absolute unity, = 100877000. The magnetic moment of the earth is therefore 8464 trillion times greater. Thus 8464 trillions of such magnet bars, with parallel magnetic axes, would be required to replace in external space the magnetic influence of the earth. Supposing the magnetism of the earth to be uniformly distributed throughout its volume, it would hence be equal to eight such bars (more exactly 7·831) for every cubic metre. This result thus enounced preserves its meaning even, if instead of

considering the earth as an actual magnet, we should prefer to ascribe terrestrial magnetism simply to constant galvanic currents in the earth. But if we consider the earth as an actual magnet, we are obliged to ascribe to each of its portions, of the size of the eighth of a cubic metre, *on an average*, at least* as great a force of magnetism as that contained in one of the abovementioned bars. Such a result will be an unexpected one to philosophers.

32.

The manner of the actual distribution of the magnetic fluids in the earth necessarily remains undetermined. In fact, according to a general theorem which has been already mentioned in the 2nd article of the *Intensitas*, and will be treated of in greater detail at a future opportunity, we may substitute for any supposed distribution of the magnetic fluids in the interior of a body occupying space, a distribution on the surface of the same space, which shall leave the effect on every point of external space precisely the same. It may be easily concluded from hence, that *one and the same* action on all external space may be deduced from an infinite number of *different* distributions of the magnetic fluids in the interior.

We are enabled to assign on the other hand that fictitious distribution on the surface of the earth, which shall be perfectly equivalent to the actual distribution in the interior, as regards the external resultant of the forces; and the spherical form of the earth allows us to do so in a very simple manner.

We may express the density of the magnetic fluid in each point of the earth's surface, i. e. the quantum of the fluid which corresponds to the unit of surface, by the formula

$$\frac{1}{4\pi}\left(\frac{V}{R}-2Z\right),$$

or by $-\frac{1}{4\pi}(3P' + 5P'' + 7P''', + 9P^{\text{IV}}, \&\text{c.})$

The result of this formula will be hereafter exhibited by a graphical representation. We shall only notice here that it is negative in the northern and positive in the southern parts of the earth, but in such manner that the dividing line cuts the

* In as far as we are not prepared to assume the magnetic axes of all the magnetized particles of the earth to be everywhere parallel to each other,—the more imperfect this parallelism, the greater must be the average force of magnetism in the parts to produce the same total magnetic moment.

equator twice (in longitudes 6° and 186°); its points most distant from the equator being in about 15° north and 15° south latitude: and further that in the northern hemisphere there are two minima, but in the southern hemisphere but one maximum. According to a cursory computation, these minima and this maximum are

$-$ 209·1 in 55° N. lat., 263° lon.
$-$ 200·0 in 71° N. lat., 116° lon.
$+$ 277·7 in 70° S. lat., 154° lon.

These values are founded on the unity of our elements, and must therefore be multiplied by 0·0034941 to obtain their expression in absolute measure.

33.

It has been already said that our elements are to be regarded only as a first approximation. So considered, their agreement with the observations in Art. 29 is sufficiently satisfactory. It cannot be doubted that a much greater agreement would be obtained by an improved calculation, even with these observations. The only difficulty of such a calculation is its length, which would be still alarmingly great, even supposing it abridged by the introduction of such skilful methods as have been employed by astronomers in correcting the elements of the planetary and cometary paths. Although this difficulty might be easily surmounted by dividing the work amongst a number of computers, it does not appear advisable to undertake such an amended calculation at present, when there is still so little certainty in the data from a great number of places which it would be important to employ. It will be preferable in the first place to pursue further the comparison of the elements with observations, whence the means will be afforded of giving much greater certainty to the general maps, than has been hitherto possible by the exclusively empirical mode.

We may be allowed to give a few glances at the future progress of the theory, the perfect realization of which may indeed be far distant.

34.

For the satisfactory refinement and completion of the elements, it will be requisite to make much higher demands than have been hitherto complied with, as to the data furnished by observation. Their accuracy at all the points employed ought

to be such as has hitherto been obtained at a very few only; they should be cleared from the effect of irregular changes; they should be all for the same epoch. It will probably be long before such demands are satisfied.

Next to this the chief desideratum is to obtain *complete* observations (i. e. including all three elements) from points in those large parts of the earth's surface where such observations are still wholly wanting. Every new station will have for the general theory an importance proportionate in great measure to its distance from those we already possess.

After a sufficient interval of time shall have elapsed, the elements may be determined afresh for a second epoch, and their secular changes may be thence deduced. Manifestly it will be essential for this purpose to reject altogether the present measure of the intensities, and to substitute for it an absolute measure.

In the course of the present century these alterations will no longer appear uniform, and the examination of the course and progress of the elements will offer to men of science inexhaustible materials for research.

35.

Conclusions as to interesting points of theory may also be expected in future.

In our theory it is assumed that every determinate magnetized particle of the earth contains precisely equal quantities of positive and negative fluid. Supposing the magnetic fluids to have no reality, but to be merely a fictitious substitute for galvanic currents in the smallest particles of the earth, this equality is necessarily part of the substitution; but if we attribute to the magnetic fluids an actual existence, there might without absurdity be a doubt as to the perfect equality of the quantities of the two fluids.

In regard to detached magnetic bodies (natural or artificial magnets), the question as to whether they do or do not contain a sensible excess of either magnetic fluid might easily be decided by very exact and delicate experiments.

In case of the existence of any such excess in a body of this nature, a plumb-line to which it should be attached should deviate from the true vertical position in the direction of the magnetic meridian.

If experiments of this kind, made with a great number of

artificial magnets and in a locality sufficiently distant from iron, never showed the slightest deviation, (which we should rather expect,) the equality of the two fluids might with the highest degree of probability be inferred for the whole earth; though without wholly excluding the possibility of some inequality.

The only difference which the existence of such an inequality would occasion in our theory would be, that P^0 (Art. 17) would no longer be $= 0$. The consequence of this would be, that for all external space it would be necessary to add to the expression for Z the member $\frac{R^2 P^0}{r^2}$; so that on the surface of the earth the (constant) member P^0 must be added, but X and Y would be in no respect affected. When there shall exist in future times a much more extensive collection of accurate observations than we at present possess, it may be examined whether a vanishing value of P^0 is or is not required for their accurate representation. With our present data such an undertaking would be wholly useless.

36.

Another part of our theory on which there may exist a doubt is, the supposition that the agents of the terrestrial magnetic force are situated exclusively in the interior of the earth. If we seek for their immediate causes, partly or wholly, without the earth, and confine ourselves to known scientific grounds, we can only think of galvanic currents. But the atmosphere is no conductor of such currents, neither is vacant space; thus, in seeking in the upper regions for a vehicle of galvanic currents we go beyond our knowledge. But our ignorance gives us no right absolutely to deny the possibility of such currents; we are forbidden to do so by the enigmatical phenomena of the Aurora Borealis, in which there is every appearance that electricity in motion performs a principal part. It will therefore still be interesting to examine what form magnetic action arising from such currents would assume on the surface of the earth.

37.

Let us, then, assume the existence of constant galvanic currents in a concave sphere, S, surrounding the earth, and call S' all the space included by S, and S'' all the space external to S. Whatever may be configuration of the galvanic currents, we can always substitute for them a fictitious distribution of the

magnetic fluids in the space S, the magnetic action of which, in all other spaces S' and S'', will be exactly similar to that of the currents.

This important proposition, which has been already mentioned (Art. 3.), rests on the following grounds: first, that these currents may be resolved into an infinite number of elementary currents (i. e. such as may be considered linear); secondly, the well-known theorem, first demonstrated, I believe, by Ampère, that in place of each linear current bounding an arbitrary surface, we may substitute a distribution of the magnetic fluids on both sides of this surface, at immeasurably small distances from it, with the same action; thirdly, the evident possibility of assigning for every re-entering line inside S, a surface bounded by it and situated wholly inside S.

If we designate by $-v$ the aggregate of all the quotients produced by dividing all the elements of the imaginary magnetic fluid by the distance of an indeterminate point, O in S' or S''; of course it is understood that the elements of the southern fluid are to be considered as negative. Then will the partial differential quotients of v, (just like those of V in our theory) express the components of the magnetic force which the galvanic currents produce at O.

38.

Although we must defer to another opportunity the detailed developement of the theory from which the proposition employed in the last article is taken, yet there is an important point relating to it which deserves to be noticed here. If we construct two different surfaces, F and F', each bounded by the same linear current G,—and (taking the simplest case for the sake of brevity) having no other point in common,—they will include a portion of space. Now, if O be situated without this space, we obtain for that constant portion of v which belongs to G, one and the same value, whether we assign the magnetic fluids to F or F'; and this value is equal to the product of the intensity of the galvanic current G (measured by an appropriate unity) multiplied by the solid angle, the summit of which is at O, and which is included by straight lines, drawn from O to the points of G; or, which is the same thing, multiplied by that portion of the spherical surface described with radius 1 round O, which is the common projection of both F and F'.

If, on the other hand, O be situated inside the space included by F and F', it is true that the two values of the part of v in question will not be the same, whether we assign the magnetic fluids to F or to F', because different parts of the spherical surface alluded to correspond to them,—which parts, taken together, make up the whole spherical surface. But as the galvanic current has opposite directions towards F and F', opposite signs must be applied in the two cases to the intensity of the current, in the multiplication into the parts of the spherical surface. The consequence is, that the algebraic difference between the values of the part of v in question is equal to the product of the intensity of the current multiplied by the whole spherical surface, or by 4π.

Hence it may easily be deduced, that if O is situated in S'', the value of v remains independent of the choice of the connecting surface; that if, on the other hand, O is situated in S', the absolute value of v does indeed depend on that choice, but the differential of v does not.

The highly fruitful theorem here touched upon,—according to which, in relation to the magnetic action of a linear galvanic current, the product of the intensity of that current, into the portion of spherical surface which is bounded by the line of current from O outwards, has the same import in regard to attracting or repelling forces, as the parts of the mass divided by the distance from O,—still requires in its generality many fuller explanations, which must be reserved for a detailed treatment of this subject.

39.

The value of v, which in general is a function of r, u, and λ, passes on the surface of the earth into a function of u and λ, and

$$-\frac{dv}{R\,du}, -\frac{dv}{R\sin u\,d\lambda}$$

are the horizontal components of the magnetic force proceeding from the galvanic currents, directed respectively towards the north and west. It is manifest that the remarkable propositions mentioned in Art. 15. and 16. hold good likewise in this case. But as to the third component, the vertical magnetic force, the case will be somewhat different, if the agents are situated above, from what it would be supposing them to be situated in the interior. To eliminate the vertical magnetic force resulting from

the former supposition, v must first be considered as a function of both r, u, and λ; it must be differentiated according to r, and then $r = R$ must be substituted.

But for the inner space S', to which the surface of the earth belongs, v can only be developed in a series according to ascending powers of r. If we make

$$\frac{V}{R} = p^0 + \frac{r}{R} \cdot p' + \frac{r^2}{R^2} \cdot p'' + \frac{r^3}{R^3} \cdot p''', \text{ \&c.}$$

p^0 is a constant magnitude, namely, the value of $\frac{V}{R}$ at the centre of the earth; p', p'', p''', &c., on the other hand, are functions of u and λ, which satisfy the same partial differential equations as p', p'', p''', above.

Hence it follows, in a similar manner to Art. 20, that the knowledge of the value of v at every point of the earth's surface is sufficient to enable us to deduce therefrom the general expression for the space S'; that we may arrive at the knowledge of this value with the exception of a constant part,—or, which is the same thing, at the knowledge of the co-efficients p', p'', p''', &c.,—by the knowledge of the horizontal forces on the surface of the earth; but that the value of the vertical force on the surface of the earth is not

$$= 2p' + 3p'' + 4p''' +, \text{ \&c.}$$

as it would be if the forces acted outwards from the interior of the earth, but is

$$= -p' - 2p'' - 3p''' -, \text{ \&c.}$$

Now, as our numerical elements (Art. 26.), determined under the supposition of the first formula, give a very satisfactory representation of the phenomena generally, whereas, the phenomena would be wholly incompatible with the second formula, the fallacy of the hypothesis, which places the causes of terrestrial magnetism in space external to the earth, may be looked upon as proved.

40.

At the same time, this must not be looked upon as decidedly disproving the possibility of a *part*, though comparatively a very small part, of the terrestrial magnetic force proceeding from the upper regions: a far more full and far more accurate knowledge of the phenomena may in future throw light on this important point of theory. If, under the supposition of

mixed causes, we attach the same meaning as before to the signs V, P°, P', P'', &c., v, p°, p', p'', applying the former to the causes acting from within, and the latter to the causes acting from without; and if we further put $V + v = W$, $P^\circ + p^\circ = \Pi^\circ$, $P' + p' = \Pi'$, $P'' + p'' = \Pi''$, &c., then on the surface of the earth,

$$\frac{W}{R} = \Pi^\circ + \Pi' + \Pi'', \text{ \&c.}$$

where $\Pi^{(n)}$ satisfies the same partial differential equation as $P^{(n)}$ (Art. 18.); and the two components of the horizontal magnetic force there existing are expressed by

$$-\frac{d\,W}{R\,d\,u}, \; -\frac{d\,W}{R\sin u\,d\,\lambda}.$$

The propositions mentioned in Articles 15. and 16. retain therefore their validity in this case, and we can determine the magnitudes Π', Π'', Π''', &c. simply from the knowledge of the horizontal forces, but without being able in any degree to conclude from hence only as to the existence of mixed causes. But if we consider the vertical force by itself, and bring it into the form

$$Q^\circ + Q' + Q'' + Q''' +, \text{ \&c.}$$

so that $Q^{(n)}$ satisfies the above-mentioned partial differences, then

$$\begin{aligned} Q^\circ &= P^\circ \\ Q' &= 2P' - p' \\ Q'' &= 3P'' - 2p'' \\ Q''' &= 4P''' - 3p''', \text{ \&c.}; \end{aligned}$$

and, consequently,

$$\begin{aligned} 3P' &= \Pi' + Q', & 3p' &= 2\Pi' - Q' \\ 5P'' &= \Pi'' + Q'', & 5p'' &= 3\Pi'' - Q'' \\ 7P''' &= \Pi''' + Q''', & 7p''' &= 4\Pi''' - Q'''. \end{aligned}$$

Thus, by the combination of the horizontal force with the vertical, we obtain the means of dividing W into its constituent parts V and v, and thus of learning whether a sensible value must be assigned to the latter. Only the constant part of v, namely, p°, is left wholly undetermined by the observations, the reason of which is plain from Art. 38.

Hence it appears important, in this interesting point of view likewise, to consider the horizontal magnetic force by itself, and we see in this an additional reason for the recommendations in Art. 21.

41.

Sufficient data for the investigation above alluded to will probably long be wanting. But it is worthy of notice, that the variations of the magnetic force, which manifest themselves simultaneously at different points of the earth's surface, are susceptible of a perfectly similar treatment. This is the case both with the regular changes corresponding to the periods of the day and of the year, and with the irregular changes. Perhaps in this way, the necessary materials may be much earlier collected. It may be well to subjoin some general remarks concerning these future researches.

After bringing the observed simultaneous changes for each place into the form of alterations of the components of the magnetic force ΔX, ΔY, ΔZ, it must first be examined whether the alterations of the two horizontal components comport themselves in correspondence with our theory, according to which ΔX and $\sin u \,.\, \Delta Y$, must be values of the partial differential quotients of a function of u and λ, according to these variables. If this is found to be the case, the conclusion will be, that the causes either are actual galvanic currents, or at least act in the same manner as such currents, or as separated magnetic fluids. In the opposite case, it would be proved that the causes cannot be galvanic currents.

We see that highly important conclusions may be derived even from the knowledge of the changes in the horizontal force only, supposing the determinations sufficiently accurate, numerous, and extensive. But if we add thereto the simultaneous changes in the vertical force, then, *supposing the first case,* the method in the preceding article will inform us whether the causes are situated above or below the surface of the earth; and further, as they are probably situated in a stratum of small thickness compared to the whole body of the earth, it may be possible to determine the mode of their propagation, at least approximatively.

As regards the second case spoken of above as possible, it certainly appears to me but little probable as concerns the regular changes in the terrestrial magnetic force depending on the time of the year or of the day. In regard to the irregular changes occurring in short intervals, I should hardly venture to pronounce a conjecture at present. If these irregular changes arise from

great electric movements above the atmosphere, it would be difficult to place these in the category of galvanic currents; for although everything seems to indicate that we should regard galvanic currents as electricity in motion, yet every movement of electricity is not a galvanic current—it is so only when the movement forms a circle returning back into itself. As it is only under this condition that it is allowable to make the often-mentioned substitution of separated magnetic fluids instead of the galvanic current, then, in the hypothesis mentioned, our relations between the components would no longer apply; that is to say, the second case would actually present itself. Even the certain establishment of this important circumstance would be in itself of great interest, and by that time we may hope to possess such extensive and accurate observations as may make it possible to trace both the source and the nature of the causes. G.

TABLE I.			TABLE I.		
	X	Z		X	Z
ϕ	a°	c°	ϕ	a°	c°
$+ 90^{\circ}$	+ 0·0	+ 1652·9	$+ 45^{\circ}$	+ 605·0	+ 1354·1
89	10·3	1652·8	44	620·7	1334·2
88	20·5	1652·7	43	636·2	1313·6
87	30·8	1652·4	42	651·5	1292·1
86	41·2	1652·1	41	666·6	1270·0
85	51·6	1651·7	40	681·5	1247·1
84	62·1	1651·1	39	696·2	1223·5
83	72·8	1650·5	38	710·6	1199·2
82	83·5	1649·7	37	724·7	1174·1
81	94·3	1648·8	36	738·5	1148·4
80	105·3	1647·7	35	752·0	1122·0
79	116·5	1646·4	34	765·2	1094·9
78	127·8	1645·0	33	777·9	1067·2
77	139·3	1643·3	32	790·3	1038·9
76	151·0	1641·4	31	802·3	1009·9
75	162·9	1639·3	30	813·9	980·5
74	175·0	1637·0	29	825·0	950·4
73	187·4	1634·3	28	835·7	919·9
72	199·9	1631·3	27	845·9	888·9
71	212·6	1628·0	26	855·7	857·4
70	225·6	1624·4	25	864·9	825·5
69	238·9	1620·3	24	873·7	793·2
68	252·3	1615·9	23	882·0	760·5
67	266·0	1611·0	22	889·8	727·5
66	279·9	1605·7	21	897·0	694·1
65	294·0	1600·0	20	903·8	660·5
64	308·3	1593·7	19	910·0	626·7
63	322·8	1586·9	18	915·8	592·6
62	337·6	1579·6	17	921·0	558·4
61	352·5	1571·7	16	925·7	523·9
60	367·6	1563·2	15	929·8	489·4
59	382·9	1554·1	14	933·5	454·8
58	398·3	1544·4	13	936·7	420·1
57	413·9	1534·0	12	939·4	385·4
56	429·6	1523·0	11	941·6	350·7
55	445·4	1511·2	10	943·3	316·0
54	461·3	1498·9	9	944·6	281·3
53	477·2	1485·8	8	945·4	246·7
52	493·3	1471·9	7	945·7	212·3
51	509·3	1457·4	6	945·7	177·9
50	525·4	1442·1	5	945·2	143·7
49	541·4	1426·0	4	944·3	109·6
48	557·4	1409·2	3	943·0	75·8
47	573·4	1391·6	2	941·4	42·1
46	589·2	1373·2	+ 1	939·4	+ 8·6
45	605·0	1354·1	0	937·1	− 24·6

TABLE I.

	X	Z
ϕ	a°	c°
0°	+ 937·1	− 24·6
− 1	934·5	57·6
2	931·5	90·3
3	928·3	122·8
4	924·8	154·9
5	921·0	186·9
6	917·0	218·5
7	912·8	249·8
8	908·4	280·8
9	903·8	311·6
10	899·1	342·0
11	894·1	372·1
12	889·1	402·0
13	883·9	431·6
14	878·6	460·8
15	873·2	489·8
16	867·7	518·6
17	862·1	547·0
18	856·4	575·3
19	850·7	603·2
20	844·9	631·0
21	839·1	658·5
22	833·2	685·7
23	827·3	712·8
24	821·4	739·7
25	815·4	766·4
26	809·3	792·9
27	803·2	819·3
28	797·1	845·5
29	790·9	871·6
30	784·7	897·5
31	778·5	923·3
32	772·1	949·0
33	765·7	974·6
34	759·3	1000·1
35	752·7	1025·5
36	746·1	1050·9
37	739·3	1076·1
38	732·5	1101·2
39	725·5	1126·3
40	718·4	1151·3
41	711·1	1176·2
42	703·7	1201·0
43	696·0	1225·8
44	688·2	1250·5
45	680·2	1275·1

TABLE I.

	X	Z
ϕ	a°	c°
− 45°	+ 680·2	− 1275·1
46	672·0	1299·5
47	663·5	1323·9
48	654·8	1348·1
49	645·9	1372·3
50	636·7	1396·2
51	627·2	1420·0
52	617·3	1443·7
53	607·2	1467·1
54	596·8	1490·3
55	586·0	1513·2
56	574·9	1536·1
57	563·5	1558·6
58	551·7	1580·8
59	539·6	1602·7
60	527·0	1624·2
61	514·1	1645·4
62	500·9	1666·1
63	487·2	1686·5
64	473·2	1706·4
65	458·8	1725·9
66	444·0	1744·9
67	428·9	1763·3
68	413·3	1781·2
69	397·4	1798·6
70	381·2	1815·3
71	364·6	1831·4
72	347·6	1846·9
73	330·3	1861·6
74	312·7	1875·7
75	294·8	1889·1
76	276·6	1901·7
77	258·1	1913·5
78	239·3	1924·6
79	220·3	1934·8
80	201·0	1944·2
81	181·6	1952·8
82	161·9	1960·5
83	142·1	1967·3
84	122·1	1973·3
85	101·9	1978·3
86	81·7	1982·5
87	61·3	1985·7
88	40·9	1988·0
89	20·5	1989·5
90	0	1989·9

TABLE II.

ϕ	X		Y		Z	
	A^{I}	$\log a^{I}$	B^{I}	$\log b^{I}$	C^{I}	$\log c^{I}$
+90°	292° 9′	2·07430	22° 9′	2·07430	172° 29′	$-\infty$
89	292 4	2·07444	22 7	2·07437	172 27	0·72139
88	291 50	2·07488	22 2	2·07458	172 20	1·02153
87	291 26	2·07563	21 54	2·07493	172 8	1·19615
86	290 52	2·07669	21 43	2·07543	171 51	1·31904
85	290 10	2·07811	21 29	2·07607	171 30	1·41333
84	289 19	2·07990	21 11	2·07686	171 3	1·48952
83	288 20	2·08211	20 51	2·07781	170 31	1·55192
82	287 14	2·08477	20 28	2·07891	169 54	1·60623
81	286 0	2·08791	20 2	2·08017	169 11	1·65259
80	284 41	2·09156	19 33	2·08160	168 22	1·69305
79	283 16	2·09573	19 2	2·08320	167 28	1·72868
78	281 46	2·10046	18 28	2·08498	166 27	1·76027
77	280 13	2·10574	17 52	2·08693	165 20	1·78844
76	278 37	2·11157	17 14	2·08906	164 6	1·81369
75	276 59	2·11794	16 34	2·09138	162 45	1·83641
74	275 20	2·12481	15 52	2·09388	161 16	1·85697
73	273 41	2·13215	15 9	2·09658	159 41	1·87567
72	272 3	2·13991	14 24	2·09945	157 57	1·89278
71	270 25	2·14803	13 37	2·10252	156 6	1·90856
70	268 50	2·15646	12 50	2·10577	154 6	1·92325
69	267 17	2·16512	12 2	2·10920	151 59	1·93709
68	265 46	2·17394	11 13	2·11280	149 44	1·95028
67	264 19	2·18288	10 24	2·11658	147 21	1·96304
66	262 56	2·19183	9 34	2·12052	144 51	1·97558
65	261 36	2·20074	8 44	2·12461	142 15	1·98809
64	260 19	2·20954	7 55	2·12885	139 33	2·00074
63	259 7	2·21816	7 5	2·13322	136 46	2·01369
62	257 58	2·22656	6 15	2·13772	133 55	2·02708
61	256 53	2·23468	5 26	2·14232	131 2	2·04101
60	255 52	2·24246	4 38	2·14703	128 8	2·05556
59	254 55	2·24986	3 50	2·15183	125 15	2·07077
58	254 1	2·25686	3 3	2·15669	122 22	2·08665
57	253 11	2·26339	2 17	2·16162	119 33	2·10318
56	252 24	2·26944	1 32	2·16659	116 48	2·12032
55	251 40	2·27497	0 48	2·17159	114 8	2·13799
54	250 59	2·27996	0 5	2·17661	111 35	2·15610
53	250 21	2·28439	359 23	2·18164	109 7	2·17456
52	249 46	2·28822	358 43	2·18666	106 47	2·19326
51	249 13	2·29145	358 3	2·19166	104 34	2·21210
50	248 43	2·29406	357 25	2·19662	102 29	2·23098
49	248 15	2·29603	356 49	2·20155	100 32	2·24979
48	247 49	2·29734	356 13	2·20641	98 42	2·26848
47	247 25	2·29799	355 39	2·21121	96 59	2·28692
46	247 3	2·39796	355 6	2·21593	95 24	2·30508
45	246 43	2·29724	354 34	2·22057	93 56	2·32288

TABLE II.

	X		Y		Z	
ϕ	A^{I}	$\log a^{I}$	B^{I}	$\log b^{I}$	C^{I}	$\log c^{I}$
+45°	246° 43′	2·29724	354° 34′	2·22057	93° 56′	2·32288
44	246 24	2·29581	354 4	2·22512	92 34	2·34027
43	246 6	2·29367	353 35	2·22956	91 18	2·35721
42	245 49	2·29080	353 7	2·23389	90 9	2·37367
41	245 34	2·28719	352 40	2·23811	89 5	2·38961
40	245 19	2·28282	352 14	2·24221	88 6	2·40502
39	245 5	2·27770	351 50	2·24618	87 12	2·41988
38	244 52	2·27179	351 26	2·25002	86 23	2·43417
37	244 39	2·26510	351 4	2·25372	85 39	2·44789
36	244 25	2·25760	350 43	2·25728	84 58	2·46103
35	244 12	2·24928	350 22	2·26071	84 22	2·47360
34	243 58	2·24012	350 3	2·26398	83 48	2·48558
33	243 44	2·23010	349 44	2·26711	83 19	2·49699
32	243 28	2·21920	349 27	2·27009	82 52	2·50782
31	243 10	2·20742	349 10	2·27292	82 28	2·51808
30	242 51	2·19471	348 54	2·27560	82 7	2·52779
29	242 30	2·18107	348 38	2·27813	81 48	2·53693
28	242 5	2·16647	348 23	2·28052	81 32	2·54554
27	241 37	2·15089	348 9	2·28275	81 18	2·55360
26	241 4	2·13431	347 55	2·28483	81 6	2·56113
25	240 26	2·11671	347 41	2·28677	80 55	2·56815
24	239 41	2·09807	347 28	2·28856	80 47	2·57465
23	238 49	2·07839	347 15	2·29021	80 39	2·58066
22	237 49	2·05768	347 3	2·29171	80 33	2·58618
21	236 37	2·03595	346 50	2·29309	80 29	2·59121
20	235 13	2·01326	346 38	2·29433	80 25	2·59578
19	233 35	1·98970	346 26	2·29544	80 22	2·59991
18	231 39	1·96540	346 14	2·29642	80 20	2·60356
17	229 23	1·94057	346 2	2·29728	80 19	2·60679
16	226 45	1·91553	345 49	2·29802	80 18	2·60959
15	223 41	1·89072	345 36	2·29865	80 17	2·61198
14	220 9	1·86675	345 23	2·29917	80 16	2·61397
13	216 7	1·84438	345 10	2·29958	80 15	2·61556
12	211 35	1·82457	344 56	2·29990	80 15	2·61677
11	206 34	1·80835	344 42	2·30014	80 13	2·61761
10	201 12	1·79678	344 27	2·30028	80 11	2·61809
9	195 33	1·79064	344 11	2·30035	80 9	2·61822
8	189 50	1·79046	343 55	2·30035	80 5	2·61802
7	184 15	1·79621	343 37	2·30029	80 0	2·61750
6	178 56	1·80737	343 19	2·30018	79 54	2·61667
5	174 3	1·82310	343 0	2·30002	79 46	2·61554
4	169 39	1·84235	342 40	2·29983	79 37	2·61414
3	165 47	1·86409	342 18	2·29961	79 25	2·61246
2	162 26	1·88741	341 56	2·29938	79 12	2·61054
1	159 34	1·91156	341 32	2·29914	78 56	2·60839
0	157 9	1·93596	341 7	2·29890	78 37	2·60603

TABLE II.

ϕ	X		Y		Z	
	A^{I}	$log\, a^{\mathrm{I}}$	B^{I}	$log\, b^{\mathrm{I}}$	C^{I}	$log\, c^{\mathrm{I}}$
0°	157° 9′	1·93596	341° 7′	2·29890	78° 37′	2·60603
− 1	155 7	1·96018	340 40	2·29869	78 15	2·60347
2	153 26	1·98393	340 12	2·29850	77 50	2·60075
3	152 3	2·00702	339 42	2·29836	77 22	2·59789
4	150 55	2·02930	339 11	2·29827	76 50	2·59491
5	150 0	2·05070	338 38	2·29824	76 14	2·59185
6	149 16	2·07116	338 3	2·29830	75 34	2·58874
7	148 41	2·09068	337 27	2·29846	74 50	2·58562
8	148 14	2·10923	336 49	2·29873	74 1	2·58252
9	147 54	2·12683	336 10	2·29912	73 8	2·57949
10	147 39	2·14348	335 29	2·29965	72 11	2·57658
11	147 28	2·15919	334 46	2·30033	71 8	2·57383
12	147 22	2·17398	334 1	2·30118	70 1	2·57129
13	147 18	2·18785	333 15	2·30222	68 49	2·56902
14	147 16	2·20083	332 27	2·30345	67 32	2·56707
15	147 16	2·21292	331 37	2·30489	66 11	2·56549
16	147 18	2·22413	330 47	2·30655	64 45	2·56435
17	147 19	2·23446	329 54	2·30845	63 15	2·56368
18	147 22	2·24391	329 1	2·31059	61 42	2·56354
19	147 24	2·25250	328 6	2·31298	60 5	2·56397
20	147 25	2·26022	327 11	2·31564	58 26	2·56499
21	147 26	2·26706	326 14	2·31856	56 44	2·56664
22	147 25	2·27302	325 16	2·32176	55 1	2·56893
23	147 23	2·27809	324 18	2·32523	53 17	2·57187
24	147 19	2·28227	323 20	2·32899	51 32	2·57546
25	147 13	2·28554	322 21	2·33302	49 47	2·57966
26	147 4	2·28790	321 22	2·33733	48 3	2·58447
27	146 52	2·28932	320 22	2·34191	46 20	2·58984
28	146 37	2·28978	319 23	2·34675	44 39	2·59572
29	146 18	2·28928	318 24	2·35186	43 0	2·60207
30	145 55	2·28780	317 25	2·35722	41 24	2·60883
31	145 27	2·28530	316 27	2·36281	39 51	2·61593
32	144 54	2·28177	315 30	2·36863	38 21	2·62331
33	144 15	2·27720	314 33	2·37467	36 55	2·63090
34	143 30	2·27156	313 37	2·38091	35 32	2·63864
35	142 37	2·26483	312 42	2·38733	34 13	2·64646
36	141 36	2·25701	311 48	2·39392	32 58	2·65430
37	140 25	2·24809	310 56	2·40066	31 46	2·66210
38	139 4	2·23808	310 4	2·40754	30 38	2·66980
39	137 30	2·22701	309 14	2·41454	29 34	2·67736
40	135 43	2·21492	308 25	2·42163	28 33	2·68471
41	133 40	2·20190	307 37	2·42882	27 36	2·69181
42	131 20	2·18809	306 51	2·43606	26 42	2·69862
43	128 39	2·17367	306 6	2·44336	25 52	2·70510
44	125 37	2·15891	305 23	2·45069	25 4	2·71121
45	122 10	2·14420	304 41	2·45804	24 19	2·71691

TABLE II.

	X		Y		Z	
ϕ	A^{I}	$log\ a^{I}$	B^{I}	$log\ b^{I}$	C^{I}	$log\ c^{I}$
−45°	122° 10′	2·14420	304° 41′	2·45804	24° 19′	2·71691
46	118 16	2·13005	304 1	2·46539	23 37	2·72218
47	113 56	2·11708	303 22	2·47272	22 58	2·72698
48	109 7	2·10605	302 44	2·48003	22 21	2·73129
49	103 53	2·09781	302 8	2·48730	21 47	2·73508
50	98 16	2·09320	301 33	2·49451	21 14	2·73833
51	92 24	2·09289	301 0	2·50166	20 44	2·74100
52	86 25	2·09739	300 28	2·50873	20 16	2·74307
53	80 27	2·10679	299 57	2·51571	19 49	2·74543
54	74 40	2·12081	299 28	2·52260	19 25	2·74453
55	69 11	2·13887	299 0	2·52937	19 1	2·74550
56	64 5	2·16018	298 33	2·53603	18 40	2·74495
57	59 25	2·18391	298 7	2·54256	18 20	2·74370
58	55 12	2·20923	297 43	2·54895	18 1	2·74169
59	51 25	2·23544	297 20	2·55521	17 43	2·73892
60	48 4	2·26198	296 57	2·56131	17 26	2·73535
61	45 4	2·28840	296 36	2·56727	17 11	2·73094
62	42 26	2·31436	296 16	2·57306	16 57	2·72566
63	40 5	2·33963	295 57	2·57868	16 43	2·71948
64	38 1	2·36405	295 39	2·58413	16 31	2·71235
65	36 10	2·38751	295 22	2·58941	16 19	2 70421
66	34 32	2·40996	295 5	2·59451	16 8	2·69503
67	33 5	2·43134	294 50	2·59942	15 58	2·68474
68	31 47	2·45165	294 35	2·60415	15 49	2·67328
69	30 37	2·47088	294 22	2·60868	15 40	2·66056
70	29 35	2·48904	294 9	2·61302	15 32	2·64650
71	28 40	2·50615	293 57	2·61716	15 24	2·63100
72	27 50	2·52223	293 45	2·62111	15 17	2·61395
73	27 5	2·53729	293 35	2·62485	15 11	2·59520
74	26 25	2·55136	293 25	2·62839	15 5	2·57459
75	25 49	2·56447	293 16	2·63172	14 59	2·55193
76	25 17	2·57662	293 7	2·63484	14 54	2·52699
77	24 48	2·58784	292 59	2·63776	14 50	2·49948
78	24 23	2·59816	292 52	2·64046	14 45	2·46904
79	24 0	2·60758	292 45	2·64296	14 42	2·43523
80	23 40	2·61613	292 39	2·64524	14 38	2·39746
81	23 22	2·62382	292 34	2·64730	14 35	2·35498
82	23 7	2·63067	292 29	2·64915	14 32	2·30676
83	22 53	2·63668	292 25	2·65079	14 30	2·25136
84	22 42	2·64187	292 21	2·65220	14 28	2·18665
85	22 32	2·64624	292 18	2·65340	14 26	2·10937
86	22 25	2·64981	292 16	2·65439	14 25	2·01401
87	22 19	2·65258	292 14	2·65515	14 24	1·89028
88	22 15	2·65456	292 13	2·65570	14 23	1·71505
89	22 12	2·65574	292 12	2·65603	14 23	1·41453
90	22 11	2·65614	292 11	2·65614	14 23	$-\infty$

TABLE III.

	X		Y		Z	
ϕ	A^{II}	$\log a^{\text{II}}$	B^{II}	$\log b^{\text{II}}$	C^{II}	$\log c^{\text{II}}$
+90°	347° 16′	$-\infty$	77° 16′	$-\infty$	176 59′	$-\infty$
89	347 15	0·60246	77 16	0·60263	176 59	9·17222
88	347 13	0·90273	77 15	0·90333	176 58	9·77385
87	347 8	1·07753	77 12	1·07889	176 56	0·12532
86	347 2	1·20066	77 9	1·20311	176 53	0·37419
85	346 54	1·29525	77 5	1·29903	176 49	0·56672
84	346 44	1·37159	77 0	1·37704	176 45	0·72351
83	346 32	1·43517	76 55	1·44260	176 40	0·83554
82	346 19	1·48927	76 48	1·49899	176 34	0·96937
81	346 3	1·53601	76 40	1·54833	176 27	1·06923
80	345 45	1·57682	76 32	1·59206	176 19	1·15802
79	345 25	1·61273	76 22	1·63121	176 10	1·23779
78	345 3	1·64451	76 12	1·66655	176 1	1·31006
77	344 39	1·67272	76 0	1·69865	175 50	1·37599
76	344 13	1·69780	75 48	1·72795	175 39	1·43647
75	343 43	1·72012	75 35	1·75483	175 27	1·49222
74	343 12	1·73995	75 20	1·77955	175 14	1·54381
73	342 38	1·75753	75 5	1·80237	175 0	1·59171
72	342 1	1·77302	74 49	1·82347	174 45	1·63630
71	341 20	1·78662	74 31	1·84301	174 29	1·67772
70	340 37	1·79844	74 13	1·86114	174 12	1·71684
69	339 51	1·80860	73 53	1·87798	173 54	1·75329
68	339 1	1·81720	73 32	1·89362	173 35	1·78747
67	338 7	1·82433	73 11	1·90815	173 14	1·81956
66	337 9	1·83005	72 48	1·92165	172 53	1·84971
65	336 6	1·83444	72 24	1·93420	172 31	1·87806
64	334 59	1·83756	71 58	1·94584	172 7	1·90472
63	333 48	1·83947	71 32	1·95663	171 42	1·92979
62	332 30	1·84022	71 4	1·96663	171 16	1·95338
61	331 7	1·83986	70 35	1·97587	170 48	1·97557
60	329 38	1·83845	70 4	1·98440	170 20	1·99642
59	328 3	1·83604	69 33	1·99224	169 50	2·01601
58	326 20	1·83270	69 0	1·99944	169 18	2·03440
57	324 29	1·82850	68 25	2·00602	168 45	2·05165
56	322 30	1·82350	67 49	2·01200	168 10	2·06780
55	320 23	1·81779	67 12	2·01743	167 34	2·08291
54	318 6	1·81148	66 33	2·02232	166 56	2·09694
53	315 39	1·80465	65 52	2·02669	166 17	2·11015
52	313 2	1·79747	65 10	2·03056	165 35	2·12237
51	310 14	1·79005	64 26	2·03396	164 52	2·13370
50	307 14	1·78257	63 41	2·03690	164 7	2·14417
49	304 4	1·77522	62 54	2·03941	163 20	2·15372
48	300 42	1·76818	62 5	2·04151	162 31	2·16267
47	297 8	1·76168	61 14	2·04320	161 40	2·17076
46	293 25	1·75593	60 22	2·04451	160 47	2·17810
45	289 31	1·75115	59 27	2·04545	159 51	2·18474

TABLE III.

	X		Y		Z	
ϕ	A^{II}	$\log a^{II}$	B^{II}	$\log b^{II}$	C^{II}	$\log c^{II}$
+45°	289° 31′	1·75115	59° 27′	2·04545	159° 51′	2·18474
44	285 30	1·74752	58 31	2·04605	158 53	2·19069
43	281 22	1·74521	57 33	2·04632	157 53	2·19598
42	277 9	1·74436	56 33	2·04627	156 50	2·20064
41	272 54	1·74504	55 30	2·04592	155 44	2·20468
40	268 38	1·74726	54 26	2·04530	154 36	2·20815
39	264 24	1·75098	53 20	2·04441	153 25	2·21106
38	260 15	1·75611	52 12	2·04328	152 11	2·21343
37	256 10	1·76251	51 1	2·04191	150 55	2·21531
36	252 13	1·77000	49 49	2·04034	149 35	2·21671
35	248 23	1·77838	48 34	2·03857	148 12	2·21766
34	244 43	1·78746	47 17	2·03662	146 46	2·21819
33	241 11	1·79704	45 28	2·03452	145 16	2·21834
32	237 49	1·80692	44 37	2·03228	143 44	2·21813
31	234 36	1·81694	43 14	2·02991	142 8	2·21759
30	231 32	1·82693	41 49	2·02744	140 29	2·21677
29	228 35	1·83676	40 22	2·02488	138 47	2·21568
28	225 47	1·84632	38 53	2·02226	137 1	2·21438
27	223 6	1·85551	37 22	2·01958	135 12	2·21287
26	220 31	1·86425	35 50	2·01686	133 20	2·21123
25	218 2	1·87248	34 15	2·01413	131 25	2·20947
24	215 38	1·88014	32 39	2·01139	129 26	2·20762
23	213 18	1·88721	31 1	2·00866	127 25	2·20572
22	211 3	1·89364	29 22	2·00595	125 21	2·20380
21	208 51	1·89942	27 41	2·00328	123 15	2·20189
20	206 42	1·90455	26 0	2·00065	121 6	2·20002
19	204 35	1·90900	24 17	1·99808	118 56	2·19821
18	202 30	1·91277	22 33	1·99557	116 43	2·19649
17	200 26	1·91588	20 48	1·99313	114 29	2·19487
16	198 23	1·91832	19 3	1·99077	112 14	2·19337
15	196 21	1·92011	17 17	1·98848	109 58	2·19199
14	194 18	1·92126	15 31	1·98626	107 41	2·19075
13	192 15	1·92179	13 44	1·98413	105 23	2·18963
12	190 12	1·92170	11 57	1·98207	103 6	2·18864
11	188 7	1·92104	10 11	1·98007	100 49	2·18776
10	186 1	1·91982	8 24	1·97815	98 33	2·18699
9	183 53	1·91806	6 38	1·97629	96 17	2·18630
8	181 43	1·91581	4 52	1·97446	94 2	2·18568
7	179 31	1·91309	3 7	1·97268	91 48	2·18510
6	177 16	1·90995	1 22	1·97092	89 36	2·18454
5	174 59	1·90641	359 37	1·96919	87 25	2·18397
4	172 38	1·90253	357 54	1·96746	85 16	2·18336
3	170 15	1·89835	356 11	1·96573	83 8	2·18269
2	167 48	1·89392	354 29	1·96397	81 3	2·18191
1	165 17	1·88929	352 48	1·96218	78 59	2·18103
0	162 43	1·88452	351 8	1·96035	76 57	2·17998

TABLE III.

	X		Y		Z	
ϕ	A^{II}	$log\ a^{II}$	B^{II}	$log\ b^{II}$	C^{II}	$log\ c^{II}$
0°	162° 43′	1·88452	351° 8′	1·96035	76° 57′	2·17998
− 1	160 6	1·87966	349 29	1·95846	74 56	2·17876
2	157 25	1·87476	347 50	1·95649	72 58	2·17733
3	154 41	1·86989	346 13	1·95444	71 1	2·17566
4	151 54	1·86509	344 36	1·95228	69 6	2·17374
5	149 4	1·86042	343 1	1·95002	67 12	2·17154
6	146 11	1·85592	341 26	1·94764	65 20	2·16905
7	143 17	1·85164	339 53	1·94512	63 29	2·16623
8	140 20	1·84762	338 20	1·94246	61 39	2·16309
9	137 22	1·84388	336 47	1·93964	59 50	2·15959
10	134 23	1·84045	335 16	1·93667	58 2	2·15573
11	131 23	1·83733	333 45	1·93352	56 15	2·15150
12	128 24	1·83452	332 14	1·93020	54 29	2·14689
13	125 25	1·83203	330 45	1·92669	52 43	2·14188
14	122 27	1·82983	329 15	1·92299	50 57	2·13648
15	119 31	1·82790	327 47	1·91910	49 12	2·13067
16	116 36	1·82621	326 18	1·91501	47 26	2·12446
17	113 44	1·82470	324 50	1·91071	45 41	2·11785
18	110 54	1·82335	323 22	1·90621	43 55	2·11083
19	108 7	1·82211	321 54	1·90150	42 9	2·10341
20	105 23	1·82091	320 26	1·89658	40 22	2·09559
21	102 43	1·81971	318 58	1·89145	38 34	2·08737
22	100 5	1·81846	317 30	1·88612	36 45	2·07878
23	97 30	1·81710	316 2	1·88057	34 56	2·06981
24	94 59	1·81560	314 34	1·87483	33 5	2·06047
25	92 31	1·81388	313 5	1·86887	31 13	2·05078
26	90 5	1·81193	311 37	1·86272	29 20	2·04076
27	87 43	1·80968	310 8	1·85637	27 26	2·03041
28	85 23	1·80711	308 38	1·84983	25 29	2·01975
29	83 5	1·80419	307 8	1·84311	23 32	2·00881
30	80 50	1·80087	305 38	1·83621	21 33	1·99760
31	78 36	1·79714	304 7	1·82913	19 32	1·98614
32	76 25	1·79296	302 35	1·82188	17 30	1·97445
33	74 14	1·78834	301 3	1·81447	15 26	1·96255
34	72 5	1·78323	299 31	1·80690	13 20	1·95047
35	69 57	1·77765	297 58	1·79919	11 14	1·93821
36	67 49	1·77157	296 25	1·79134	9 6	1·92581
37	65 42	1·76499	294 51	1·78335	6 57	1·91327
38	63 35	1·75791	293 16	1·77524	4 47	1·90061
39	61 27	1·75034	291 41	1·76701	2 37	1·88785
40	59 19	1·74228	290 6	1·75866	0 26	1·87498
41	57 10	1·73373	288 31	1·75020	358 14	1·86202
42	55 0	1·72472	286 55	1·74163	356 3	1·84896
43	52 49	1·71526	285 19	1·73297	353 52	1·83580
44	50 37	1·70537	283 43	1·72420	351 42	1·82252
45	48 23	1·69506	282 7	1·71533	349 33	1·80912

TABLE III.

	X		Y		Z	
ϕ	A^{II}	$log\ a^{II}$	B^{II}	$log\ b^{II}$	C^{II}	$log\ c^{II}$
−45°	48° 23′	1·69506	282° 7′	1·71533	349° 33′	1·80912
46	46 7	1·68438	280 31	1·70636	347 25	1·79558
47	43 49	1·67335	278 56	1·69729	345 18	1·78186
48	41 29	1·66199	277 21	1·68810	343 13	1·76793
49	39 7	1·65036	275 47	1·67880	341 10	1·75376
50	36 42	1·63848	274 13	1·66937	339 10	1·73931
51	34 16	1·62640	272 40	1·65981	337 12	1·72452
52	31 47	1·61415	271 8	1·65009	335 17	1·70935
53	29 17	1·60177	269 37	1·64021	333 25	1·69375
54	26 45	1·58929	268 7	1·63013	331 35	1·67764
55	24 11	1·57675	266 39	1·61985	329 50	1·66098
56	21 37	1·56417	265 12	1·60933	328 7	1·64368
57	19 2	1·55158	263 47	1·59855	326 28	1·62568
58	16 26	1·53898	262 23	1·58747	324 52	1·60691
59	13 51	1·52638	261 2	1·57607	323 21	1·58728
60	11 17	1·51376	259 42	1·56430	321 52	1·56672
61	8 44	1·50111	258 25	1·55212	320 27	1·54513
62	6 13	1·48839	257 9	1·53949	319 6	1·52242
63	3 45	1·47556	255 56	1·52635	317 48	1·49850
64	1 20	1·46254	254 46	1·51265	316 34	1·47326
65	358 58	1·44928	253 37	1·49834	315 24	1·44658
66	356 40	1·43567	252 31	1·48335	314 17	1·41834
67	354 27	1·42163	251 28	1·46760	313 13	1·38840
68	352 19	1·40704	250 27	1·45101	312 12	1·35661
69	350 15	1·39176	249 29	1·43351	311 15	1·32281
70	348 18	1·37567	248 34	1·41498	310 21	1·28680
71	346 25	1·35860	247 41	1·39531	309 30	1·24837
72	344 39	1·34039	246 51	1·37437	308 42	1·20727
73	342 59	1·32084	246 3	1·35202	307 57	1·16322
74	341 25	1·29975	245 18	1·32808	307 16	1·11588
75	339 56	1·27687	244 36	1·30235	306 37	1·06485
76	338 34	1·25192	243 57	1·27458	306 0	1·00966
77	337 18	1·22457	243 21	1·24448	305 27	0·94972
78	336 8	1·19443	242 47	1·21167	304 56	0·88472
79	335 4	1·16100	242 16	1·17572	304 28	0·81256
80	334 5	1·12370	241 47	1·13602	304 3	0·73327
81	333 13	1·08172	241 22	1·09181	303 40	0·64493
82	332 26	1·03401	240 59	1·04207	303 19	0·54547
83	331 45	0·97911	240 39	0·98533	303 1	0·43201
84	331 10	0·91487	240 21	0·91948	302 46	0·30031
85	330 40	0·83802	240 6	0·84123	302 33	0·04380
86	330 16	0·74302	239 54	0·74509	302 22	9·95118
87	329 57	0·61958	239 45	0·62075	302 14	9·70281
88	329 44	0·44456	239 38	0·44509	302 8	9·35148
89	329 35	0·14417	239 34	0·14432	302 5	8·74992
90	329 33	$-\infty$	239 33	$-\infty$	302 3	$-\infty$

TABLE IV.

	X		Y		Z	
ϕ	A^{III}	$log\ a^{III}$	B^{III}	$log\ b^{III}$	C^{III}	$log\ c^{III}$
+90°	221° 48′	—∞	311° 48′	—∞	36° 0′	—∞
89	221 48	8·41399	311 48	8·41408	36 0	6·83649
88	221 50	9·01555	311 49	9·01591	36 1	7·73926
87	221 52	9·36689	311 50	9·36770	36 2	8·26700
86	221 54	9·61559	311 52	9·61702	36 4	8·64106
85	221 58	9·80790	311 54	9·81013	36 6	8·93082
84	222 2	9·96441	311 57	9·96763	36 8	9·16719
83	222 8	0·09612	312 0	0·10050	36 11	9·36663
82	222 14	0·20957	312 3	0·21530	36 15	9·53899
81	222 21	0·30901	312 8	0·31627	36 19	9·69062
80	222 29	0·39732	312 12	0·40629	36 23	9·82585
79	222 37	0·47655	312 17	0·48742	36 28	9·94777
78	222 47	0·54824	312 23	0·56119	36 34	0·05867
77	222 57	0·61353	312 29	0·62875	36 40	0·16026
76	223 9	0·67331	312 36	0·69100	36 46	0·25391
75	223 21	0·72831	312 43	0·74864	36 53	0·34068
74	223 34	0·77908	312 50	0·80226	37 1	0·42143
73	223 49	0·82611	312 59	0·85232	37 9	0·49686
72	224 4	0·86977	313 7	0·89922	37 17	0·56756
71	224 20	0·91040	313 17	0·94327	37 26	0·63402
70	224 38	0·94825	313 26	0·98476	37 36	0·69664
69	224 56	0·98357	313 37	1·02392	37 46	0·75579
68	225 16	1·01656	313 48	1·06095	37 57	0·81266
67	225 37	1·04739	313 59	1·09603	38 8	0·86482
66	225 59	1·07620	314 11	1·12930	38 20	0·91520
65	226 22	1·10314	314 23	1·16091	38 32	0·96309
64	226 47	1·12831	314 37	1·19098	38 45	1·00868
63	227 13	1·15183	314 50	1·21961	38 59	1·05213
62	227 40	1·17377	315 5	1·24689	39 13	1·09356
61	228 9	1·19422	315 20	1·27290	39 28	1·13312
60	228 39	1·21325	315 35	1·29773	39 43	1·17090
59	229 11	1·23093	315 51	1·32144	39 59	1·20702
58	229 45	1·24732	316 8	1·34409	40 16	1·24157
57	230 21	1·26246	316 26	1·36574	40 34	1·27462
56	230 58	1·27641	316 44	1·38644	40 52	1·30626
55	231 37	1·28922	317 3	1·40624	41 11	1·33655
54	232 19	1·30091	317 22	1·42517	41 30	1·36556
53	233 2	1·31152	317 42	1·44329	41 51	1·39345
52	233 48	1·32110	318 3	1·46062	42 12	1·41996
51	234 36	1·32967	388 25	1·47720	42 34	1·44546
50	235 26	1·33726	318 47	1·49306	42 57	1·46990
49	236 19	1·34390	319 10	1·50823	43 20	1·49327
48	237 15	1·34960	319 34	1·52274	43 45	1·51567
47	238 14	1·35441	319 58	1·53661	44 10	1·53711
46	239 16	1·35835	320 24	1·54987	44 36	1·55764
45	240 21	1·36143	320 50	1·56254	45 3	1·57728

TABLE IV.

	X		Y		Z	
ϕ	A^{III}	$\log a^{III}$	B^{III}	$\log b^{III}$	C^{III}	$\log c^{III}$
+45°	240° 21′	1·36143	320° 50′	1·56254	45° 3′	1·57728
44	241 30	1·36369	321 17	1·57464	45 31	1·59606
43	242 43	1·36514	321 44	1·58619	46 0	1·61401
42	243 59	1·36581	322 13	1·59721	46 30	1·63116
41	245 19	1·36574	322 42	1·60771	47 1	1·64754
40	246 44	1·36494	323 13	1·61772	47 33	1·66317
39	248 13	1·36344	323 44	1·62725	48 6	1·67807
38	249 47	1·36129	324 16	1·63631	48 40	1·69226
37	251 26	1·35850	324 49	1·64493	49 15	1·70578
36	253 11	1·35513	325 23	1·65311	49 51	1·71862
35	255 1	1·35122	325 57	1·66087	50 29	1·73083
34	256 57	1·34681	326 33	1·66822	51 7	1·74241
33	258 59	1·34196	327 9	1·67518	51 47	1·75338
32	261 8	1·33672	327 47	1·68175	52 28	1·76376
31	263 23	1·33116	328 25	1·68796	53 10	1·77356
30	265 45	1·32535	329 5	1·69380	53 54	1·78283
29	268 13	1·31937	329 45	1·69930	54 39	1·79154
28	270 49	1·31330	330 27	1·70446	55 25	1·79974
27	273 31	1·30722	331 9	1·70930	56 12	1·80742
26	276 21	1·30123	331 52	1·71382	57 1	1·81462
25	279 17	1·29542	332 37	1·71804	57 51	1·82134
24	282 19	1·28988	333 22	1·72197	58 43	1·82759
23	285 28	1·28470	334 8	1·72561	59 36	1·83341
22	288 42	1·27997	334 56	1·72898	60 30	1·83879
21	292 1	1·27576	335 44	1·73208	61 26	1·84375
20	295 24	1·27214	336 33	1·73493	62 23	1·84832
19	298 50	1·26916	337 23	1·73754	63 21	1·85250
18	302 19	1·26686	338 14	1·73991	64 21	1·85630
17	305 50	1·26524	339 6	1·74206	65 23	1·85975
16	309 21	1·26430	339 59	1·74399	66 25	1·86286
15	312 52	1·26403	340 53	1·74570	67 30	1·86563
14	316 22	1·26438	341 48	1·74722	68 35	1·86809
13	319 51	1·26530	342 43	1·74855	69 42	1·87025
12	323 17	1·26672	343 40	1·74969	70 50	1·87212
11	326 41	1·26859	344 37	1·75065	71 59	1·87372
10	330 1	1·27080	345 35	1·75145	73 9	1·87505
9	333 19	1·27328	346 33	1·75208	74 21	1·87613
8	336 32	1·27595	347 32	1·75255	75 34	1·87698
7	339 43	1·27873	348 32	1·75287	76 47	1·87759
6	342 49	1·28156	349 33	1·75305	78 2	1·87799
5	345 53	1·28435	350 34	1·75309	79 17	1·87818
4	348 54	1·28706	351 35	1·75299	80 34	1·87816
3	351 51	1·28963	352 37	1·75276	81 51	1·87796
2	354 47	1·29201	353 39	1·75241	83 8	1·87757
1	357 40	1·29418	354 42	1·75193	84 26	1·87700
0	0 31	1·29611	355 45	1·75132	85 45	1·87626

TABLE IV.

ϕ	X: A^{III}	X: $log\ a^{III}$	Y: B^{III}	Y: $log\ b^{III}$	Z: C^{III}	Z: $log\ c^{III}$
0°	0° 31′	1·29611	355° 45′	1·75132	85° 45′	1·87626
1	3 21	1·29778	356 47	1·75060	87 3	1·87535
2	6 10	1·29918	357 51	1·74976	88 22	1·87426
3	8 58	1·30030	358 54	1·74880	89 41	1·87301
4	11 46	1·30115	359 57	1·74772	91 0	1·87159
5	14 34	1·30175	1 0	1·74652	92 19	1·87000
6	17 22	1·30211	2 3	1·74520	93 38	1·86824
7	20 11	1·30226	3 6	1·74376	94 56	1·86630
8	23 0	1·30223	4 9	1·74219	96 14	1·86418
9	25 51	1·30205	5 11	1·74049	97 31	1·86187
10	28 43	1·30176	6 13	1·73867	98 48	1·85936
11	31 36	1·30140	7 14	1·73670	100 4	1·85665
12	34 30	1·30103	8 15	1·73460	101 19	1·85373
13	37 26	1·30068	9 16	1·73234	102 33	1·85058
14	40 23	1·30041	10 16	1·72994	103 47	1·84720
15	43 21	1·30025	11 15	1·72737	104 59	1·84357
16	46 20	1·30026	12 14	1·72464	106 10	1·83968
17	49 19	1·30047	13 12	1·72174	107 20	1·83552
18	52 19	1·30091	14 9	1·71865	108 29	1·83107
19	55 18	1·30160	15 6	1·71537	109 36	1·82632
20	58 16	1·30258	16 1	1·71189	110 42	1·82125
21	61 14	1·30384	16 56	1·70820	111 47	1·81585
22	64 9	1·30539	17 50	1·70430	112 51	1·81010
23	67 3	1·30722	18 43	1·70017	113 53	1·80398
24	69 54	1·30931	19 35	1·69580	114 53	1·79749
25	72 42	1·31164	20 27	1·69118	115 53	1·78960
26	75 27	1·31417	21 17	1·68630	116 51	1·78329
27	78 8	1·31685	22 6	1·68115	117 47	1·77555
28	80 45	1·31964	22 54	1·67572	118 42	1·76737
29	83 17	1·32249	23 42	1·67000	119 36	1·75872
30	85 45	1·32535	24 28	1·66398	120 28	1·74958
31	88 7	1·32816	25 13	1·65763	121 19	1·73995
32	90 25	1·33087	25 58	1·65096	122 8	1·72979
33	92 38	1·33340	26 41	1·64395	122 56	1·71909
34	94 46	1·33572	27 23	1·63658	123 43	1·70784
35	96 49	1·33776	28 4	1·62884	124 28	1·69601
36	98 46	1·33947	28 45	1·62072	125 12	1·68358
37	100 39	1·34081	29 24	1·61220	125 54	1·67053
38	102 27	1·34172	30 2	1·60327	126 36	1·65684
39	104 10	1·34215	30 40	1·59391	127 16	1·64249
40	105 49	1·34208	31 16	1·58411	127 55	1·62745
41	107 24	1·34145	31 51	1·57385	128 32	1·61171
42	108 54	1·34022	32 26	1·56312	129 9	1·59523
43	110 20	1·33836	32 59	1·55188	129 44	1·57800
44	111 42	1·33584	33 31	1·54014	130 18	1·55998
45	113 0	1·33262	34 3	1·52785	130 52	1·54115

TABLE IV.

ϕ	X A^{III}	X $\log a^{III}$	Y B^{III}	Y $\log b^{III}$	Z C^{III}	Z $\log c^{III}$
−45°	113° 0′	1·33262	34° 3′	1·52785	130° 52′	1·54115
46	114 15	1·32867	34 34	1·51502	131 23	1·52147
47	115 26	1·32395	35 3	1·50161	131 54	1·50092
48	116 34	1·31844	35 32	1·48759	132 24	1·47945
49	117 39	1·31210	36 0	1·47296	132 53	1·45705
50	118 40	1·30491	36 27	1·45767	133 21	1·43365
51	119 39	1·29681	36 54	1·44170	133 48	1·40924
52	120 35	1·28780	37 19	1·42502	134 14	1·38376
53	121 28	1·27783	37 44	1·40761	134 39	1·35716
54	122 19	1·26686	38 7	1·38942	135 3	1·32940
55	123 7	1·25486	38 30	1·37041	135 26	1·30043
56	123 53	1·24178	38 53	1·35055	135 48	1·27017
57	124 37	1·22759	39 14	1·32980	136 10	1·23857
58	125 19	1·21223	39 35	1·30810	136 31	1·20556
59	125 59	1·19566	39 54	1·28541	136 50	1·17106
60	126 36	1·17782	40 14	1·26166	137 9	1·13498
61	127 12	1·15865	40 32	1·23680	137 28	1·09724
62	127 46	1·13808	40 50	1·21076	137 45	1·05774
63	128 19	1·11603	41 6	1·18346	138 2	1·01635
64	128 49	1·09244	41 23	1·15481	138 18	0·97296
65	129 18	1·06719	41 38	1·12473	138 33	0·92742
66	129 46	1·04019	41 53	1·09311	138 48	0·87957
67	130 12	1·01132	42 7	1·05982	139 2	0·82925
68	130 36	0·98045	42 21	1·02473	139 15	0·77624
69	130 59	0·94743	42 34	0·98770	139 28	0·72031
70	131 21	0·91208	42 46	0·94854	139 40	0·66122
71	131 42	0·87421	42 57	0·90705	139 51	0·59864
72	132 1	0·83357	43 8	0·86299	140 1	0·53223
73	132 19	0·78990	43 19	0·81610	140 11	0·46157
74	132 36	0·74286	43 28	0·76604	140 21	0·38618
75	132 52	0·69208	43 37	0·71242	140 30	0·30547
76	133 7	0·63709	43 46	0·65478	140 38	0·21874
77	133 20	0·57730	43 53	0·59254	140 45	0·12512
78	133 32	0·51202	44 1	0·52498	140 52	0·02356
79	133 44	0·44034	44 7	0·45122	140 59	9·91270
80	133 54	0·36110	44 13	0·37009	141 5	9·79081
81	134 3	0·27280	44 19	9·28007	141 10	9·65560
82	134 11	0·17337	44 24	0·17911	141 15	9·50400
83	134 19	0·05992	44 28	0·06431	141 19	9·33165
84	134 25	9·92822	44 32	9·93144	141 22	9·13223
85	134 30	9·77171	44 35	9·77395	141 25	8·89588
86	134 34	9·57941	44 37	9·58084	141 28	8·60613
87	134 38	9·33071	44 39	9·33151	141 30	8·23208
88	134 40	8·97937	44 41	8·94136	141 31	7·70435
89	134 41	8·37781	44 42	8·33933	141 32	6·80158
90	134 42	− ∞	44 42	− ∞	141 32	− ∞

TABLE V.

	X	Y	Z
	A^{IV} = 142° 26′	B^{IV} = 232° 26′	C^{IV} = 322° 26′
ϕ	$\log a^{IV}$	$\log b^{IV}$	$\log c^{IV}$
+ 90°	− ∞	− ∞	− ∞
89	6·04417	6·04423	4·38300
88	6·94686	6·94713	5·58686
87	7·47447	7·47507	6·29078
86	7·84836	7·84942	6·78992
85	8·13790	8·13956	7·17676
84	8·37399	8·37637	7·49252
83	8·57310	8·57635	7·75916
82	8·74509	8·74933	7·98980
81	8·89629	8·90167	8·19291
80	9·03103	9·03768	8·37426
79	9·15241	9·16047	8·53797
78	9·26271	9·27231	8·68709
77	9·36366	9·37493	8·82393
76	9·45660	9·46969	8·95028
75	9·54260	9·55766	9·06756
74	9·62252	9·63968	9·17693
73	9·69707	9·71617	9·27932
72	9·76682	9·78862	9·37551
71	9·83226	9·85659	9·46615
70	9·89381	9·92082	9·55179
69	9·95181	9·98166	9·63290
68	0·00656	0·03940	9·70988
67	0·05833	0·09430	9·78309
66	0·10734	0·14661	9·85283
65	0·15379	0·19651	9·91937
64	0·19786	0·24419	9·98295
63	0·23969	0·28981	0·04377
62	0·27943	0·33350	0·10202
61	0·31720	0·37538	0·15786
60	0·35311	0·41558	0·21146
59	0·38725	0·45419	0·26294
58	0·41972	0·49130	0·31242
57	0·45059	0·52700	0·36001
56	0·47993	0·56135	0·40583
55	0·50781	0·59444	0·44994
54	0·53428	0·62633	0·49245
53	0·55941	0·65706	0·53343
52	0·58323	0·68669	0·57295
51	0·60579	0·71528	0·61107
50	0·62713	0·74287	0·64787
49	0·64728	0·76950	0·68335
48	0·66628	0·79520	0·71762
47	0·68415	0·82002	0·75071
46	0·70092	0·84398	0·78266
45	0·71661	0·86712	0·81352

TABLE V.

	X	Y	Z
	A^{IV} = 142° 26′	B^{IV} = 232° 26′	C^{IV} = 322° 26′
ϕ	$\log a^{IV}$	$\log b^{IV}$	$\log c^{IV}$
+ 45°	0·71661	0·86712	0·81352
44	0·73124	0·88947	0·84332
43	0·74483	0·91105	0·87209
42	0·75740	0·93189	0·89987
41	0·76895	0·95201	0·92670
40	0·77950	0·97143	0·95260
39	0·78905	0·99018	0·97759
38	0·79761	1·00827	1·00171
37	0·80518	1·02571	1·02497
36	0·81176	1·04254	1·04741
35	0·81735	1·05876	1·06904
34	0·82195	1·07439	1·08988
33	0·82555	1·08944	1·10994
32	0·82814	1·10393	1·12926
31	0·82970	1·11786	1·14784
30	0·83023	1·13126	1·16570
29	0·82970	1·14413	1·18286
28	0·82808	1·15647	1·19932
27	0·82536	1·16831	1·21510
26	0·82149	1·17965	1·23022
25	0·81644	1·19050	1·24468
24	0·81017	1·20086	1·25850
23	0·80263	1·21075	1·27168
22	0·79374	1·22017	1·28424
21	0·78345	1·22912	1·29619
20	0·77168	1·23763	1·30752
19	0·75832	1·24568	1·31826
18	0·74327	1·25329	1·32840
17	0·72639	1·26046	1·33796
16	0·70753	1·26719	1·34695
15	0·68650	1·27370	1·35535
14	0·66306	1·27938	1·36320
13	0·63693	1·28484	1·37047
12	0·60776	1·28988	1·37720
11	0·57511	1·29451	1·38337
10	0·53839	1·29872	1·38898
9	0·49686	1·30253	1·39406
8	0·44948	1·30593	1·39859
7	0·39482	1·30892	1·40258
6	0·33075	1·31151	1·40604
5	0·25400	1·31370	1·40896
4	0·15908	1·31549	1·41134
3	0·03568	1·31688	1·41320
2	9·86069	1·31788	1·41452
1	9·56033	1·31847	1·41531
0	− ∞	1·31867	1·41558

TABLE V.

	X	Y	Z
	$A^{IV} =$ 322° 26′	$B^{IV} =$ 232° 26′	$C^{IV} =$ 322° 26′
ϕ	$log\ a^{IV}$	$log\ b^{IV}$	$log\ c^{IV}$
0°	$-\infty$	1·31867	1·41558
− 1	9·56033	1·31847	1·41531
2	9·86069	1·31788	1·41452
3	0·03568	1·31688	1·41320
4	0·15908	1·31549	1·41134
5	0·25400	1·31370	1·40896
6	0·33075	1·31151	1·40604
7	0·39482	1·30892	1·40258
8	0·44948	1·30593	1·39859
9	0·49686	1·30253	1·39406
10	0·53839	1·29872	1·38898
11	0·57511	1·29451	1·38337
12	0·60776	1·28988	1·37720
13	0·63693	1·28484	1·27047
14	0·66306	1·27938	1·36320
15	0·68650	1·27350	1·35535
16	0·70753	1·26719	1·34695
17	0·72639	1·26046	1·33796
18	0·74327	1·25329	1·32840
19	0·75832	1·24568	1·31826
20	0·77168	1·23763	1·30752
21	0·78345	1·22912	1·29619
22	0·79374	1·22017	1·28424
23	0·80263	1·21075	1·27168
24	0·81017	1·20086	1·25850
25	0·81644	1·19050	1·24468
26	0·82149	1·17965	1·23022
27	0·82536	1·16831	1·21510
28	0·82808	1·15647	1·19932
29	0·82970	1·14413	1·18286
30	0·83023	1·13126	1·16570
31	0·82970	1·11787	1·14784
32	0·82814	1·10393	1·12926
33	0·82555	1·08944	1·10994
34	0·82195	1·07439	1·08988
35	0·81735	1·05876	1·06904
36	0·81176	1·04254	1·04741
37	0·80518	1·02571	1·02497
38	0·79761	1·00827	1·00171
39	0·78905	0·99018	0·97759
40	0·77950	0·97143	0·95260
41	0·76895	0·95201	0·92670
42	0·75740	0·93189	0·89987
43	0·74483	0·91105	0·87209
44	0·73124	0·88947	0·84332
45	0·71661	0·86712	0·81352

TABLE V.

	X	Y	Z
	$A^{IV} =$ 322° 26′	$B^{IV} =$ 232° 26′	$C^{IV} =$ 322° 26′
ϕ	$log\ a^{IV}$	$log\ b^{IV}$	$log\ c^{IV}$
− 45°	0·71661	0·86712	0·81352
46	0·70092	0·84398	0·78266
47	0·68415	0·82002	0·75071
48	0·66626	0·79520	0·71762
49	0·64728	0·76950	0·68335
50	0·62713	0·74287	0·64785
51	0·60579	0·71528	0·61107
52	0·58323	0·68669	0·57295
53	0·55941	0·65706	0·53343
54	0·53428	0·62633	0·49245
55	0·50781	0·59444	0·44994
56	0·47993	0·56135	0·40583
57	0·45059	0·52700	0·36001
58	0·41972	0·49130	0·31242
59	0·38725	0·45419	0·26294
60	0·35311	0·41558	0·21146
61	0·31720	0·37538	0·15786
62	0·27943	0·33350	0·10202
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64	0·19786	0·24419	9·98295
65	0·15379	0·19651	9·91937
66	0·10734	0·14661	9·85283
67	0·05833	0·09430	9·78309
68	0·00656	0·03940	9·70988
69	9·95181	9·98166	9·63290
70	9·89381	9·92082	9·55179
71	9·83226	9·85659	9·46615
72	9·76682	9·78862	9·37551
73	9·69707	9·71647	9·27932
74	9·62252	9·63968	9·17693
75	9·54260	9·55766	9·06756
76	9·45660	9·46969	8·95028
77	9·36366	9·37493	8·82393
78	9·26271	9·27231	8·68709
79	9·15241	9·16047	8·53797
80	9·03103	9·03768	8·37426
81	8·89629	8·90167	8·19291
82	8·74509	8·74933	7·98980
83	8·57310	8·57635	7·75916
84	8·37399	8·37637	7·49252
85	8·13790	8·13956	7·17676
86	7·84836	7·84942	6·78992
87	7·17447	7·47507	6·29078
88	6·94686	6·94713	5·58686
89	6·01417	6·01423	4·38300
90	$-\infty$	$-\infty$	$-\infty$

Article VI.

*On a new Instrument for the direct Observation of the Changes in the Intensity of the Horizontal Portion of the Terrestrial Magnetic Force**. By Carl Friedrich Gauss.

[From the *Resultate aus den Beobachtungen des magnetischen Vereins im Jahre* 1837.—Herausgegeben von Carl Friedrich Gauss und Wilhelm Weber. Göttingen, 1838.]

It is well known that for the perfect determination of the terrestrial magnetic force at a given place, *three* elements are required; and, in general, the Declination, Inclination, and Intensity are selected for the purpose. Although this choice is the most simple in conception, it is not only allowable, but in many respects it may be advisable, to adopt a different combination. In practical as well as in theoretical respects, it is far more advantageous to consider the horizontal portion of the terrestrial force separately, and to imagine it in two elements, the direction (declination) and the intensity. If we add to these, as a third element,—either the intensity of the vertical force, or the inclination,—the intensity of the total force, if desired, may be directly obtained.

With respect to the two elements of the horizontal force, with which alone we are here concerned, all the questions which occur in regard to the *declination* are completely met by the magnetometer which has been in use since 1833†. This instrument serves with a certainty, convenience, and accuracy that leaves nothing more to be wished; not only for the determination of the absolute value of the declination, but also for following its regular and accidental changes, from year to year, from month to month, from hour to hour,—nay, from one minute to the other. This magnetometer also determines, in absolute measure, the *intensity* of the horizontal portion of the earth's magnetic force,—which was, in fact, the object which first gave rise to its construction: it does not, however, by any means, solve this problem *perfectly* in *all* respects.

The application of the magnetometer to determine the magnetic intensity is founded on a combination of *several* operations,

* Translated by Mr. William Francis, and revised by Major Sabine and Professor Lloyd.

† Scientific Memoirs, part v. pp. 25 *et seq.*

one of which consists in observing the time of vibration of a needle. But this operation, from its very nature, requires a considerable time, as the number of vibrations from which we deduce the duration of a single vibration ought not to be too small. Now, supposing the magnetic intensity to be constant during the period employed in the observation, the resulting time of vibration will correspond truly to the intensity; but if the latter has varied in the interval, the time of vibration will only correspond to its mean value. Whatever changes may have taken place during the interval are entirely concealed from us, the instrument giving only average values. If, in order to approximate more closely, we were to choose shorter intervals, or to base the results upon a smaller number of vibrations, we should sacrifice accuracy and certainty, and be in danger of considering errors of observation as anomalies in the intensity.

But the more interesting the magnetic disturbances in short intervals appeared,—as shown by the experiments of last year, in regard to the declination only,—the more important it was to possess a means by which the effects of similar disturbances in the intensity might be followed and measured with the same ease, certainty, and accuracy.

We have already seen that the unfitness of the method hitherto employed for this purpose consists in the circumstance, that it is based on observations of the times of vibration, which, from their very nature, must always require a long interval. Now the time of vibration serves in this case only to determine indirectly the moment of rotation which the earth's magnetism imparts to the needle when it is not situated in the magnetic meridian. If, then, we can determine accurately this moment of rotation in a direct manner without observations of vibration, and if we can measure its variation with accuracy, quickness, and certainty, our main object is attained. The method to be described for this purpose rests on the following basis.

The necessary conditions of equilibrium of a body of any form suspended to *two threads*,—its parts being supposed, in the first instance, subject to gravity alone, and firmly connected with each other,—may be thus briefly described: the vertical passing through the centre of gravity of the body, and the straight lines coinciding with the threads, are in one plane, and are either parallel with each other, or intersect in a fixed point. In all

cases, therefore, in the position of equilibrium, the two threads and the centre of gravity are in one vertical plane. To give precision to our ideas, it may be supposed that the two threads are of equal length; that the two upper points of connexion are at the same height, and that their distance apart is the same as that of the two lower points; and lastly, that the two latter form with the centre of gravity an equiangular triangle. Under these suppositions, in a state of equilibrium, the two threads will hang vertically, and a third vertical line, midway between them, will pass through the centre of gravity of the body. If we remove the body from this position by means of a rotation around the last-named line, the two threads will no longer be vertical, nor will they be in one plane, and at the same time the body will be somewhat raised. There arises consequently a tendency to return to the former position, with a moment of rotation, which may, with sufficient accuracy, be regarded as proportional to the sine of the deviation from the position of rest, and which is, therefore, greatest when the deviation amounts to 90 degrees. This maximum effect is always understood when the moment of rotation is spoken of; it may also be regarded as the force by which the body is retained in equilibrium by its mode of suspension, and which, for shortness, I shall call the directive force of suspension. The magnitude of this force depends, 1st, on the length of the suspending threads; 2nd, on their distance apart; 3rd, on the weight of the body; being inversely proportional to the length of the threads, and directly as the square of their distance apart, and as the weight of the body. If the above suppositions are not fulfilled, the expression for the directive force is more complicated, and the reaction of the threads against the torsion also renders a small modification necessary. Means are not wanting to enable us to determine by experiment, with the greatest accuracy, the magnitude of the directive force. If the body is left to itself, after having been made to deviate from its position of equilibrium, it will vibrate with the greatest regularity, the middle of the vibrations coinciding with this position, and the duration depending on the magnitude of the directive force and on the moment of inertia of the body.

If we further suppose a horizontal magnet bar to form a part of the suspended body, a second directive force is exerted, and the phænomena depend on the combinations of the two forces,

according to the known laws of statics. There are, in this point of view, three cases to be distinguished, according as the two positions of the body, in which it would be in a state of equilibrium arising from either of the two forces acting singly, either coincide,—or are opposite,—or form an angle with each other. It is easily seen that the difference between these three cases rests on the relation of the two angles, which the straight line joining the two lower points of connexion of the thread forms with the magnet bar; and which the straight line joining the two upper points of suspension forms with the magnetic meridian. If we imagine the body in that position of equilibrium which is due solely to the mode of suspension, the magnet bar must be, in the first of our three cases, in the magnetic meridian, and in its natural position (*i. e.* the north pole towards the north); in the second case, it must be in the magnetic meridian, but in the reverse position; and in the third case, it must form an angle with the magnetic meridian. For the sake of brevity, I will call these three positions of the magnet bar, the direct, the reverse, and the transverse positions.

In the direct position, the action of terrestrial magnetism on the magnet bar does not change the position of equilibrium corresponding to the mode of suspension; but the apparatus is retained in the same position by an increased force, which is the sum of the two directive forces.

In the reverse position, the equilibrium does not cease, but it is only stable when the magnetic directive force is smaller than the directive force arising from the mode of suspension; and the body is then only retained in this position by a force which is the difference between the two directive forces. If the magnetic directive force were the greater, the equilibrium would be unstable, and the body once disturbed from that position would not return to it, but would depart further and further from it, and only come to rest in the opposite position, in which the bar is in its natural position in space, but the suspending threads cross one another.

Finally, in the third case, where the two directive forces form an angle with each other, the conflict of these forces will end in an intermediate position, where, on the one hand, the bar will not be in the meridian, and, on the other, a straight line through the lower points of connexion of the threads will not be parallel to a straight line through the upper points. This intermediate

position, and the force by which the apparatus is retained in it, obey the statical laws of the composition of two forces. It will now be easily seen, that if the apparatus presents the means of measuring the angle between the three positions in question, the relation of the two component directive forces may be calculated, and consequently, we can express in absolute measure the magnetic directive force, if the force arising from the mode of suspension is also known in absolute measure. Thus our problem is solved. It is most advantageous to dispose the magnet bar relatively to the other parts of the apparatus, so that it shall form, in the mean position of equilibrium, nearly a right angle with the magnetic meridian, to which case the term *transverse* position is chiefly applicable. By this means, the deviation of the threads from their position in one plane is greatest, and the result is therefore most accurate; and a small change in the magnetic declination, arising from hourly or accidental fluctuations, has no perceptible influence on the position. On the contrary, every change in the intensity of terrestrial magnetism affects the position directly, and can at once be recognised and measured with the same ease, quickness, and accuracy as the changes of declination are by the ordinary magnetometer.

I had many years ago ascertained the practical applicability of this mode of determination, by preliminary experiments with an apparatus (very rough it is true) to which I have alluded in my Memoir on Terrestrial Magnetism and on the Magnetometer. Recently, however, I have had a more perfect apparatus constructed, and have suspended it in the astronomical observatory, in the spot previously occupied by the magnetometer with the bar of 25lbs. weight. After what has been said, a few words will suffice for the description of this apparatus. It is suspended by two steel wires 17 feet in length, or, to speak more accurately, by a single one, the extremities of which are attached to the apparatus beneath, whilst, above, the centre of the wire passes over two cylinders which keep it at a proper distance apart (about $1\frac{1}{2}$ inch); by this arrangement the two wires have, of themselves, an equal tension. The suspension is above the ceiling of the room, and the wires hang freely through a circular aperture ($3\frac{1}{2}$ inches wide) in the ceiling. The interval of the wires, both above and below, can be increased or diminished at pleasure. The apparatus suspended to the wires consists of four principal parts. The first, to which the wires

are affixed, is an horizontal circular disk, 4 inches in diameter, divided on silver into quarter degrees. The second part consists of an alidade, concentric with the circle, and rotating on its limb, and having two verniers indicating minutes; a strong rod, perpendicular to the plane of the circle, is firmly connected with the alidade, and to this is fixed a very perfect circular mirror $1\frac{1}{2}$ inch in diameter, in which may be seen, through a telescope placed at the distance of 16 feet, the image of a portion of an horizontal scale, divided in millimetres, fixed below the telescope. In this manner every change in the position of the circle may be seen and measured; small changes directly, and with great accuracy by the divisions of the scale seen in the telescope; and greater ones by combining a movement of the alidade and reading off the verniers. The third portion of the apparatus is a stirrup situated beneath the circle, being a double frame, in which the fourth constituent part, a 25-pound magnet bar, is inserted. This stirrup has likewise a rotatory motion round the centre of the circle, and is provided with two indexes applied to the limb of the circle, by which the amount of the rotation can be measured to a minute.

If now we place the stirrup so that the apparatus preserves the same position of equilibrium, whether the magnet bar be in the stirrup, or a non-magnetic body of equal weight, we have the first or the second of the positions distinguished above, according as the magnet bar is in the direct or in the reverse position. The first affords no particularly important practical application; and the advantage of the second is connected with the condition, that the magnetic directive force be somewhat less than the directive force due to the mode of suspension. With our apparatus the proportion of these forces is at present nearly as ten to eleven: the resulting directive force is consequently only the tenth part of the magnetic directive force. We have consequently in this case an arrangement analogous to an astatic magnetic needle; and every extraneous force that disturbs the direction of the ordinary needle is indicated here by a tenfold greater effect than would take place in the case of suspension by a single thread; and, as will be easily perceived, in the opposite direction. This then affords, among other things, the solution of a problem which has been often attempted without success, viz. that of representing the daily and hourly changes of the magnetic declination under a magnified form. Numerous simultaneous observations of this kind, made with this appa-

ratus, and with the magnetometer of the observatory, have afforded the most satisfactory results. This application, however, has lost much of its importance from the introduction of the declination magnetometer, which gives the minutest changes with all the accuracy that can be desired.

This and other applications of the instrument, with the bar in the reverse position, to which I shall hereafter return, must, however, be considered as of minor importance; the employment of the apparatus in the third or transverse position for observations of the intensity being far more important. If in proceeding from the direct position the magnetic bar is deflected from the magnetic meridian by turning the stirrup, the whole apparatus, in order to regain its equilibrium, must turn back through a certain angle corresponding to the proportion of the two directive forces; the difference of the two angles will be the deviation of the magnetic bar from the magnetic meridian when in the position of equilibrium; and it may easily be arranged so that this deviation shall amount to nearly 90 degrees, and thus the advantages previously spoken of be gained. In this position the apparatus is peculiarly well adapted for observing changes of the intensity, which are immediately indicated by changes of position. In regard to such changes as only take place in long intervals, several circumstances must be attended to; for instance, it is requisite that from time to time we examine, by known and appropriate means, whether and to what extent the magnetism of the bar may have changed; the variations of temperature must also be considered, both in their effect on the magnetic state of the bar, and on the interval and length of the suspending wires, and, consequently, on the directive force arising from the mode of suspension. But with respect to the irregular changes of the intensity in short intervals, this apparatus performs the same service as the magnetometer does in respect to similar changes of the declination; and the mode of observation with both instruments is the same. The changes of intensity are obtained, expressed in parts of the scale, which, however, may easily be reduced to fractions of the intensity itself. Under the present relations of the apparatus, the 22,000th part of the entire intensity answers to one division of the scale.

The experiments hitherto made with the apparatus, comprising as yet but a very short period, have already indicated some important results.

In the first place, the observations indicate the regular changes

dependent on the time of day, which, it is true, are as frequently intermixed with irregular ones as in the declination; to discriminate between them with certainty will require observations continued for years. If I may venture, from the very little experience hitherto gained, to express a supposition rather than a result, the regular change seems to consist in this,—that the intensity decreases in the hours of the forenoon, so that it attains its minimum one or two hours before mid-day, and then again increases. In order, however, to obtain provisionally the quantitative ratio, I have noted the position in the morning at 10, and in the afternoon at 3, on thirty days in August, 1837. The result was, that on twenty-six days, the intensity was greater in the afternoon than in the morning, and less on only four days; the mean difference amounting to 39 parts of the scale, or somewhat more than the 600th part of the entire intensity. On most of these days the apparatus was also noted in the morning at 9 o'clock; of twenty-eight days, there were twenty-three on which the intensity was still at this hour greater than an hour later, and the reverse was found to be the case on five days only; the mean difference, however, amounted in this case only to $11\frac{1}{2}$ divisions of the scale, or somewhat more than the 2000th part of the entire intensity.

Secondly, several very extensive series of observations prove that irregular, and, at times, very considerable disturbances, and varying in short intervals of time, occur not less frequently in the intensity than in the declination, as analogy would have led us to expect. Uninterrupted series of some length have been made on these occasions simultaneously with the intensity apparatus, and with the magnetometer of the observatory; 1st, on the 15th July, 1837, from 6 A.M. to 6 P.M.; 2ndly, in the usual magnetic term of the 29th and 30th July; and 3rdly, during the extra term of the 31st August and 1st September; the observations being made at every five minutes. In comparing the two series, it is observable, that where the declination was violently disturbed, in general great disturbances also occur in the intensity*.

By the representation of the changes of the declination and of the intensity in two distinct curves, as is done for the November term in the first Part, Plate XIII., we are far from obtaining

* In a similar way, and with the same result, observations were also subsequently made with both apparatus in the term of 13th to 14th November.

so perfect an image of the course of the disturbances as by their combination in a single curve. A complete representation of the terrestrial magnetic force (*i. e.*, its horizontal portion) at each moment, is given by a single straight line, of which the length is proportional to the intensity, and the angle which it forms with a fixed line is equal to the declination. To represent the force in several successive moments, the same point of commencement of the different straight lines is preserved, and the terminal points alone exhibited; these are noted with the corresponding times, and are united by a line (Plate XIII, part 2). The radii themselves are not drawn, and even the common point of commencement, to obtain anything like a convenient scale, must always be situated far beyond the drawing. This leads us to a new point of view, from which we may consider such changes of the two magnetic elements. In fact, they are the two horizontal components of that always comparatively small disturbing force, to which the mean terrestrial magnetic force is at each moment subject, resolved into two directions—one in the magnetic meridian, and the other perpendicular to it. The second component is given directly by the magnetometer, the first by the new apparatus; for which reason both must be reduced to a common measure before the drawing.

In applying this very illustrative mode of representation, it must be remembered that the course of the changes during a whole day cannot be represented in one drawing without confusion, if there are frequent and quickly varying changes, as the curve would present too many convolutions: it is necessary, therefore, in such case, to make separate drawings for shorter intervals.

If we compare the new apparatus and the magnetometer, we find that the two, with respect to *some purposes*, serve reciprocally to render each other complete; but, in other respects, have one and the same application. For the determination of the absolute declination, the magnetometer alone is applicable, and not the new apparatus. The changes of the declination, and especially the quickly varying changes, may be followed with both. For determining the absolute intensity, both apparatus may be employed, although the use of the magnetometer is somewhat less complicated than the sole employment of the new instrument would be; but the former of itself can only give the mean value of the intensity for a certain interval, and the quickly

varying changes are entirely overlooked with this instrument, while the new instrument indicates them most satisfactorily. For all other purposes—for instance, for comparing magnetic bars with one another in respect to their magnetic strength,—and in connexion with a multiplier, for galvanometrical and telegraphical purposes,—both are alike useful. With respect to the two last applications, the new apparatus has an important advantage, in its being in our power, as above mentioned, to render it as nearly astatic as may be desired. A few instances of the sensibility of the apparatus as a galvanometer may be here noticed. The multiplier surrounding the magnetic bar contains 610 coils of copper wire covered with silk, and the galvanic current has to pass through a length of wire of more than 6000 feet. This length increases to 13,000 feet if the current at the same time is brought from the physical cabinet. In general, however, other apparatus are brought into connexion with the chain, so that in many experiments the whole length of wire amounts to 40,000 feet, or nearly two German miles. By far the greatest portion of this wire is very thin; and this length, in so far as the force of the current is affected by it, is equivalent to a wire about eight German miles in length, of the thickness of the connecting wire between the Astronomical Observatory and the Physical cabinet. Notwithstanding this long chain, even the weakest galvanic forces give the heavy magnetic bar a deflection not merely perceptible, but sufficing for accurate measurements. This applies to thermo-galvanism, respecting which many philosophers have the erroneous notion that it cannot pass through a very long chain. With the arrangements at Göttingen, and on the application of a thermo-galvanic apparatus of peculiar construction, the effect is produced merely by touching the connecting points with the finger. The application to the common electricity of friction gives rise to another interesting observation. It is known that Colladon discovered, by experiments which were at first doubted, but subsequently confirmed by Faraday, that the common electricity of friction, conducted through a multiplier, deflects the needle in the same manner as a hydro-galvanic current. Faraday was the first to prove that, in a very powerful electrical battery, no more electricity is developed than very weak hydro-galvanic means of excitation propel in a few seconds through a conducting wire of moderate length. Both the reality and the small amount of the electro-

magnetic action of machine electricity were experimentally confirmed several years ago with the apparatus here; it appeared, however, worth while to repeat these experiments with the aid of the new and much more sensitive apparatus. Instead of discharging a Leyden jar, or a battery of jars, by a wire chain (as Colladon and Faraday did), only the conductor and the rubber of an electric machine in the Physical Cabinet were connected with the wire chain passing to the Astronomical Observatory, which was 13,000 feet in length, including the multiplier. The electrical machine was then turned with uniform velocity; when this was done with a velocity of one revolution to a second, the five-and-twenty-pound magnet bar in the new apparatus in the Astronomical Observatory was thereby kept at a deflection corresponding to 144 parts of the scale (somewhat more than 50 minutes),—the deflection being positive or negative according to the direction in which the current passed through the multiplier. The experiments showed as much regularity as could be wished. But the circumstance especially remarkable is that the electro-magnetic effect remained the same even when the length of the chain was increased to above a German mile by the introduction of other apparatus. This might seem to be an essential difference from other currents, excited either hydro-galvanically, thermo-galvanically, or by induction; the intensity of which, indicated by the magnitude of the electro-magnetic effects, becomes constantly smaller the longer the conducting apparatus; but I find in it a striking confirmation of the theory, according to which the unequal intensity, indicated by the unequal electro-magnetic action of two galvanic currents, is nothing more than the unequal quantity of electricity passing through each section of the conducting apparatus in a fixed time. With other modes of excitation, a given electromotive force developes less electricity in a given time, the greater the opposition made to the current by the longer chain; in our experiment, on the contrary, the quantity of electricity in motion depends merely on the play of the machine, and all electricity passing to the conductor in the form of sparks must traverse the whole chain, be it long or short, in order to equalize itself with the opposite electricity from the rubber.

In order to demonstrate the preference due to the new apparatus over the magnetometer in electro-magnetic telegraphy, we must first consider somewhat more closely the

manner of producing telegraphic signs, by means of galvanic currents.

As soon as it was known that the action of a voltaic pile propagated itself through a very long chain, the inference seemed obvious that these natural forces might be employed for telegraphic purposes; and, thirty years ago*, when but a small portion of the galvanic actions were known, Sömmering proposed the evolution of gas for the purpose. The magnetic actions of galvanic currents, which were subsequently discovered, are far better adapted for complicated signals; yet it is surprising, that since Oersted's discovery, many years elapsed before any one seems to have thought of applying them to this purpose. It is true that it was not possible to form a well-grounded opinion of their applicability on a large scale, without an accurate quantitative knowledge of the decrease in force of galvanic currents, resulting from the length and quality of the conducting wires; on which subject, before Ohm and Fechner, very imperfect and erroneous notions were entertained. With a view chiefly to the performance, on a large scale, of similar experiments on the law of the force of galvanic currents under different circumstances, a connecting wire was established in 1833 between the Astronomical Observatory and the Physical Cabinet; the merit of the execution of this difficult project is due to Prof. Weber. This chain was from the first frequently employed for telegraphic signals; not merely for simple ones in the daily comparison of the clocks, but complicated signals were also tried for the sake of experiment, and the possibility of communicating letters, words, and whole phrases, was even then an ascertained fact†. In these experiments an hydro-galvanic current was employed, excited only by very weak means, viz. a single or a double pair of plates, and unacidulated water; I shall not, however, stop here to describe the method then employed, as I have since substituted for it one entirely different. In the first method there was this inconvenience;

* I have learnt from a note communicated to me by von Humboldt, that Bétancourt had, ten years ago, laid down a wire from Aranjuez to Madrid, by means of which telegraphic signals could be effected by the discharge of a Leyden flask. Although no detailed account of the result appears to be known, there can be no doubt of the success of such an experiment, if properly performed; but such a method must always have been limited to conveying an affirmative or negative reply to a few previously-concerted questions.

† The first public notice of these experiments is in the *Gött. gelehrten Anzeige*, 1834, p. 1273. See Schumacher's *Jahrbuch* for 1836, p. 38.

that with our simple chain and the arrangement of the apparatus then adopted, (such experiments being merely a subordinate object) no more than two letters could be signaled in a minute. Even with the new arrangement expressly formed for the purpose, this velocity (which is obviously unconnected with the length of the chain, or the distance apart of its extremities,) could not be considerably increased as long as only a simple chain was employed, though it would be increased to a very high degree with a compound one; but there was no sufficient reason in this case for establishing a chain of the latter kind, as there could be no doubt as to the result, and its real scientific value would have borne no proportion to the expense.

But the laws of induction have led me to a quite different method, in which a simple chain has been employed for more than two years, with complete success, for a much more rapid telegraphing. It will be the more allowable to dwell longer on this subject, as hitherto I have published no details concerning it. I have elsewhere described*, many years ago, the apparatus which I term inductor. I must however remark, that instead of the inductor of 1050 coils described in the first notice, and of that subsequently increased to 3537 coils, the present one consists of 7000 coils,—the length of the wire alone amounting to more than 7000 feet. By a very simple manipulation with this inductor, (viz. by removing it quickly from a double magnet bar, on which it is at first placed, and then bringing it back immediately to its former position, without reversing it,) two powerful opposite galvanic currents are caused to pass through the conducting wire, one quickly after the other, and each lasting only an extremely short time. The effect of these two currents upon a magnet bar surrounded by a multiplier, and situated anywhere in the chain, consists in this: that it produces for a moment a very quick velocity, which is immediately destroyed. The needle, therefore, makes a very rapid but small movement either to the right or to the left, according as may be desired, and then is immediately at rest. It is evident that the changes of such rapid movements may be combined in various ways, and may be employed for signaling letters. Some degree of practice will of course be required to give the signals rapidly and precisely on the one hand, and on the other to read them with ease and certainty; but even by unpractised

* *Gött. gelehrten Anzeige*, 1835, p. 351; Schumacher's *Jahrbuch* for 1836, p. 41.

persons, about seven letters can easily be signaled in one minute, as many experiments have shown. If, instead of manipulation, appropriate mechanical arrangements were adopted, the velocity and precision would undoubtedly be considerably increased.

It is precisely in this kind of telegraphing that the new apparatus possesses a considerable advantage over the magnetometer; and for the following reasons. Although the two opposite impulses, of which one simple signal consists, are exactly *equal* in *force*,—and consequently the second destroys just as much velocity as the first produced,—yet the needle cannot be in perfect quiescence between the signals, because this perfect quiescence is only possible when the needle is in its natural position of equilibrium. Even if it is in this position *previous* to a signal, it is somewhat disturbed therefrom by the signal itself, and the directive force acting on the needle causes it to tend to return. Though a single signal causes only a very slight movement, yet a considerable disturbance from the natural position of equilibrium will arise from the accumulation of a great number of signals; and the result will be so much motion between the signals that they will lose somewhat of their sharpness of expression. It will easily be seen, on consideration, that under circumstances otherwise similar, this disadvantage is greater when the needle employed has a short interval of vibration, than when it has a long one. Its effect is greater, therefore, on the magnetometer in the Magnetic Observatory, than on the 25-lb. needle suspended in the Astronomical Observatory; and is least of all on the new apparatus, when its magnet bar, by being placed in the reverse position, is converted almost into an astatic needle. Thus, even when the needle is at a considerable distance from its position of equilibrium, the comparatively weak directive force, with which it tends to return to that position, does not produce in it any movements which can materially disturb the signals, while the current in the multiplier acts as strongly on the needle, and consequently produces quite as rapid movements, as if it belonged to an ordinary magnetometer.

A peculiar apparatus, which I have lately caused to be constructed, is highly useful in preventing the disadvantages and inconveniences arising from untimely vibratory motions, both in this kind of telegraphing, and in many other applications of magnetic apparatus. I give it the name of a *damper*, as its action con-

sists in entirely destroying, in a very short time, vibratory motions, which would otherwise be continued for several hours. The damper constructed at first for the magnetometer in the magnetic observatory produced this effect in a very high degree, so that the greatest vibratory motions disappeared entirely in a few minutes. A similar arrangement can be applied to any vibrating needle, to the magnetometer, and to the new apparatus here treated of; and will certainly form an essential part of every apparatus which is to be employed for telegraphing by the method described above. A more complete explanation of this apparatus would, however, lead us too far from our present subject.

No particular name has been as yet given to the new apparatus. From its chief application it might be termed an *Intensitometer*. But as it is applicable to as many and as accurate magnetic measurements as the magnetometer, it has perhaps an equal claim to the name. The essential difference is, that the new apparatus is suspended by *two* threads, by which a new directive force is obtained with which the magnetic force is commensurable. The other differences, viz. in the mode of attaching the mirror, and in the means of measuring the relative amount of rotation of the several parts of the instrument, are conditions necessarily arising from the objects to be obtained. The new apparatus may therefore be termed a *bifilar* or *bipensil-magnetometer*, to distinguish it from the older instrument, the simple or *unifilar* magnetometer. I may express my conviction, that its more extended use, and especially its employment, conjointly with the declination magnetometer, in the term observations, at stations widely remote from each other, will be soon followed by an important progress in our knowledge of the disturbances of the earth's magnetism. GAUSS.

[The graphical representations of the changes in the direction and intensity of the horizontal force at Göttingen in the terms of July 29–30, Aug. 31—Sept. 1, and Nov. 13–14, 1837, referred to in the preceding memoir, page 259, are contained in the *Resultate aus den Beob.* for 1837. As our purpose is rather that of illustration than of record, it has appeared sufficient to give one of these plates; and we have selected for the purpose that of the term of November 13–14, appointed expressly on those days, on account of the great number of *falling stars* which had been

observed in them in preceding years. Part 1, Plate XIII., represents the changes of the Intensity in the upper line, and of the Declination in the lower. The justice of the remark, in p. 259, will be at once recognised, namely, that, when considerable changes take place in the one element, they ar usually accompanied by considerable changes in the other. Part 2 represents the changes of both elements united in one curve, and affords an illustrative delineation of the variation of the horizontal portion of the earth's magnetic force. To avoid the confusion arising from too repeated involutions of the curve, it is divided into three separate portions, and in each of these half the curve is drawn in an unbroken line, and half in a dotted line.

M. Gauss remarks, that "the observations during this term of Nov. 13–14 do not present greater disturbances than had been noticed in many of the terms at other seasons of the year. On the preceding and following evenings, very great and rapidly-varying changes took place in the declination; but these are known to be the general accompaniments of the Aurora Borealis, which was extremely brilliant on those two nights."—EDIT.]

Article VII.

Observations on the Arrangement and Use of the Bifilar Magnetometer. By Wilhelm Weber*.

[From the *Resultate aus den Beobachtungen des magnetischen Vereins im Jahre* 1837.—Herausgegeben von Carl Friedrich Gauss und Wilhelm Weber. Göttingen, 1838.]

AFTER the full development in the preceding article of the principle of the Bifilar Magnetometer, and of all that is essentially necessary for its construction and application, an exact drawing of the instrument will be particularly interesting. The drawing (Plate XIV.) is so accurate that any skilful artist can work from it. The following observations are added with a view of rendering the drawing still more intelligible, and of facilitating the adjustment of the instrument by other observers, as far as may be done by such directions.

1. *General Observations.*

The height and other dimensions of the Göttingen astronomical observatory, where the instrument figured has been established, allowed of large size in the instrument, and therefore a 25lb. bar very powerfully magnetized was employed. At other places it will perhaps be necessary to employ smaller dimensions, and we shall notice at the conclusion the difference in the cost produced by diminishing the size. Large dimensions are generally, however, more to be recommended for the bifilar than for the unifilar, for the following reasons: 1st, because no proportionate increase in the price is occasioned, as the principal expense arises from the fine division of the circle and from the mirror; and since the latter is not attached to the end of the magnet bar, it is not requisite that its size should be increased with that of the bar; 2nd, because the enlargement of the instrument does not require any considerable enlargement of the room; which would be the case with the unifilar, on account of the experiments of deflection in the measurements of the absolute intensity; 3rd, because the magnet bar need very rarely be removed from the stirrup; and therefore the size of the bar pro-

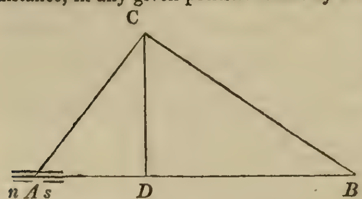
* Translated by Mr. William Francis, and revised by Professor Lloyd and Major Sabine.

duces no inconvenience in its use, which would be the case to a certain extent with the other magnetometer. It does not follow that a bar of exactly five-and-twenty-pounds' weight, such as we have used, must be employed; one of ten pounds will suffice for the most delicate measurements, and even one of four pounds might answer. The small bars have only one advantage over larger ones, in the greater facility of imparting to them a strong degree of magnetism; and this is only of importance where powerful means of exciting magnetism by friction are wanting.

With respect to a suitable locality, a room similar to that employed for the unifilar is all that is requisite, even if a bar of 25lbs. is employed. The breadth of the room may even be less, and its length may form any angle with the magnetic meridian, because the mirror in this case is not attached to the extremity of the magnetic bar, but to the stirrup at the centre of the bar, and it can be turned in any direction. A considerable height is requisite, so that the interval of the two threads or wires to which the instrument is suspended may be sufficient for convenient measurement without rendering the directive force too great. As it is rare that a room is sufficiently high, it is advisable to pierce the ceiling, and to carry the wires as high as the roof will allow. In regard to the height, it is of little consequence whether a heavy or light bar be placed in the stirrup, supposing only both bars to be proportionally magnetized, and both to be much heavier than the stirrup. It is not necessary to construct a separate building free from iron for the bifilar, as is done for the other magnetometer; it may be placed, as is the case at Göttingen, in the middle of a room in a building from which iron has not been excluded: it is sufficient to remove all iron from the immediate neighbourhood of the instrument: it is best, however, to place it in the magnetic observatory which contains the other magnetometer, if the room is large enough and adapted for the purpose. If, for instance, the changes of the declination and of the intensity are to be observed *simultaneously* during the terms, a double number of observers is necessary if the apparatus are in different buildings. But if both are in *one* large room, and so arranged that, whilst the magnetometers are at a sufficient distance asunder, the theodolites with which the observations are made are situated near one another, one clock may serve both observers, and one practised observer may observe alternately with both instruments, allowing an interval

of two minutes. The two magnetometers may be so placed relatively to each other in a large room, that the mean declination may remain unaltered, and the changes of the declination and of the intensity be only so far affected, that the determination of the value of the divisions of the scale is somewhat different from what it would otherwise have been. This is the case when the pillar supporting the theodolites forms with the two magnetometers a triangle, of which one side (viz. that between the pillar and the declination-magnetometer) is situated in the magnetic meridian, while the other side, viz. the line which connects the central points of the two magnetometers, forms an angle of 35° 15′ 52″ with the magnetic meridian*. The

* Prof. Gauss has given, in a very simple geometrical construction, the complete solution of the problem of the reciprocal action of two magnets at a great distance, in any given position relatively to each other. It is as follows:



Let A be the centre of a small magnet, $n\,s$; $A\,B$ the prolongation of $n\,s$; C a particle of free magnetism of the other bar; $A\,C\,B$ a right angle; $AD = \frac{1}{3}\,AB$; then *$C\,D$ is the direction of the force* which acts upon C, when C is a north magnetic particle; (when C is a south magnetic particle, the direction of the force is, on the contrary, in the prolongation of $D\,C$ beyond C) $\frac{C\,D}{A\,D} \cdot \frac{M\,m}{A\,C^3}$ is *the magnitude of the force*, M designating the magnetism of $n\,s$, and m the magnetism at C. This simple proposition, which is useful in numberless cases, is especially applicable to this case, in which the most advantageous reciprocal position of the magnetometers to be placed in the same room is required; *i. e.* that position in which they will least disturb each other, and in which, whatever slight disturbance may be produced can easily be brought into calculation as a correction. The application of Gauss's proposition to our case shows that in the position above described, 1st, the *mean* declination remains unchanged; 2nd, the value of the divisions of the scale, not only for the *variations* of the declination, but also of the intensity, are only altered in so far as the *directive force* of the two apparatus undergoes a change; for the value of the divisions of the scale changes with the directive force, and in the same proportion. This may all be seen from the geometrical construction of the reciprocal action of two magnets at a great distance, without its being necessary to give a detailed development of *the theory* of the two magnetometers.

The *first* assertion is evident from the consideration of the above figure, where A is the central point of the intensity-bar $n\,s$, C the central point of the declination-bar situated in the line $C\,D$, $C\,D$ the magnetic meridian, and where the straight line $A\,C$, which connects the centres of the two bars, forms the angle $A\,C\,D = 35°\ 15'\ 52''$ with the magnetic meridian $C\,D$,—or, more accurately, forms such an angle, $A\,C\,D$, that

$$\sin A\,C\,D = \sqrt{\tfrac{1}{3}}$$
$$\operatorname{cotan} A\,C\,D = \sqrt{2}$$
$$\operatorname{cosec} A\,C\,D = \sqrt{3}$$

According to the above proposition, $C\,D$ is the *direction* of the force which

height of suspension, which is of such great importance for the objects of this instrument, renders it very desirable that access to the points of suspension should be rarely or never required.

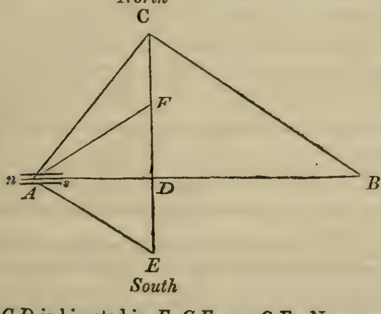
acts on the declination-bar C, for if $A\,C\,B = 90°$, $A\,D = \frac{1}{3}\,A\,B$. This latter case is the actual one, because $C\,D$ is perpendicular to $A\,B$ (the magnetic axis of the declination-bar must be situated in the magnetic meridian, and the magnetic axis $A\,B$, of the intensity-bar must be placed perpendicularly to it); then $A\,C$ being the half diameter, $A\,D$ is the sine of $A\,C\,D$, $A\,B$ the secant of $B\,A\,C$, or the cosecant of $A\,C\,D$; consequently,

$$A\,D : A\,B = \sin A\,C\,D : \operatorname{cosec} A\,C\,D = \sqrt{\tfrac{1}{3}} : \sqrt{3} = 1 : 3.$$

The direction of the force with which the intensity-bar acts on the declination-bar is therefore that of the magnetic meridian $C\,D$: it may consequently have some influence on the time of vibration of the declination-bar, the directive force of which is somewhat changed by it; but it will exert no influence on its direction, so long as this direction coincides with the assumed *mean* meridian $C\,D$: the *deviations* from it will, however, be somewhat diminished or increased by this force, according as it acts conjointly with, or in opposition to, the terrestrial magnetic force; but even this is provided for if we alter the value in arc of the divisions of the scale, in which the *deviations from the mean meridian* are expressed proportionately to the force of direction, *i. e.* by $\frac{\sqrt{2}}{A\,C^3} \cdot \frac{M\,m}{T\,m}$, where, according to the above proposition, $\frac{M\,m}{A\,C^3} \cdot \sqrt{2} = \frac{M\,m}{A\,C^3} \cdot \cot A\,C\,D$ $= \frac{C\,D}{A\,D} \cdot \frac{M\,m}{A\,C^3}$, designates the *magnitude* of the directive force produced by the intensity-bar, and $T\,m$ designates the directive force of the earth.

The *latter* assertion, in so far as it relates to the changes of the intensity, is proved by letting fall a perpendicular on $C\,A$ at C, which intersects the prolongation of the line $C\,D$ at E. It then results from the similarity of the triangles $A\,C\,D$, $A\,B\,C$, $E\,A\,D$, $E\,C\,A$, that $E\,D = \frac{1}{3}\,E\,C$, because $A\,D$ was equal to $\frac{1}{3}\,A\,B$;

North



South

consequently, if $C\,D$ is bisected in F, $C\,F = \frac{1}{3}\,C\,E$. Now, as all that has been said of A, $A\,B$, C, $A\,B\,C$, $A\,D$, and $C\,D$, is true also of C, $C\,E$, A, $C\,A\,E$, $C\,F$, and $A\,F$, it results that $A\,F$ is the *direction*, and $\frac{A\,F}{C\,F} \cdot \frac{M\,m}{A\,C^3}$ is the *magnitude* of the force with which the declination-bar acts on the intensity-bar. If now

Even in the construction of the Unifilar Magnetometer it was noticed that it would be convenient that the torsion circle, of which frequent use is made, should be fixed to the stirrup of the magnetometer instead of to the ceiling. The same object has been considered in the construction of the bifilar, where, on account of the greater height of suspension, it was of much more consequence. To make it quite unnecessary to go to the ceiling in the case of the bifilar, several other arrangements are requisite at the stirrup. The screws, for instance, which serve to lengthen and shorten the wires, have to be fixed to the stirrup instead of the ceiling. They are very clearly marked in Pl. XIV., figs. 1, 2, 3, where it is seen how they are connected with the circle on which the stirrup is placed, and arranged in the same way as the elevating screw of the unifilar, so that the wires may be lengthened or shortened without any lateral displacement.

It is also necessary to be able to bring the two wires closer to, or further from each other, at the stirrup, so as to increase or diminish their directive force at pleasure. Although it is most simple that the two wires which support the instrument should be always equi-distant above and below, and that whenever it

we resolve this force into a force acting in its magnetic axis, by multiplying the entire force by the fraction $\frac{AD}{AF}$, and into a force perpendicular to it (towards the magnetic meridian) by multiplying the entire force by the fraction $\frac{DF}{AF}$, we obtain for the *first* the value

$$\frac{AD}{AF} \cdot \frac{AF}{CF} \cdot \frac{Mm}{AC^3} = \frac{AD}{CF} \cdot \frac{Mm}{AC^3} = \frac{Mm}{AC^3} \cdot \sqrt{2};$$

and for the latter the value

$$\frac{DF}{AF} \cdot \frac{AF}{CF} \cdot \frac{Mm}{AC^3} = \frac{DF}{CF} \cdot \frac{Mm}{AC^3} = \frac{Mm}{AC^3},$$

since $2\,CF = CD = AD\sqrt{2}$, or $\frac{AD}{CF} = \sqrt{2}$, and $CF = DF$.

The force $\frac{Mm}{AC^3}\sqrt{2}$ directly changes the directive force of the intensity-bar in its transversal position. This force $\frac{Mm}{AC^3}$ would alter its *position* if the effect were not counteracted by a suitable change in the suspension, so that the bar should remain unmoved in its transversal position. In the latter case it is true that the force $\frac{Mm}{AC^3}$ no longer comes into consideration; but the changed suspension has certainly some influence on the directive force, and consequently on the value of the divisions of the scale. This, however, does not require any separate calculation, being included in the calculation of the directive force from the given suspension,—a problem which belongs to the *Theory of the Bifilar Magnetometer*, which will subsequently be developed.

is desired to increase or diminish the directive force, they should be moved through an equal quantity at both extremities, it is by no means necessary. The change in the interval of the wires may be effected below only, but in such case to a greater degree. The apparatus figured is, in fact, so arranged, that, with a mean distance of the upper ends, every necessary increase or diminution of the directive force can be produced by a displacement of the suspension screws at the stirrup; however, for the sake of completeness, the apparatus is provided with an arrangement at top for an equal displacement of the two cylinders, over which the wire is conducted, and by which its two vertical suspended ends are kept separate from each other; so that, if it is desired, the upper distance may always be rendered equal to the lower. In case it is not desired to retain the power of making this upper displacement, these *two cylinders* may be united into a *roller* of a suitable diameter, and the axis of this roller, like that of a friction wheel, may be allowed to run on wheels, so as to diminish the friction, and cause the two wires to have an equal tension;—a point which is of great importance in absolute determinations.

2. *On the separate parts of the Bifilar.*

The description of these is reduced almost wholly to a description of the stirrup, because it unites nearly all the parts which in the unifilar are distributed among the stirrup, the ceiling, and the extremity of the bar. It is also unnecessary to speak of the theodolite and its stand, the clock, the scale, or the mark, as all these have been treated of in the account of the former instrument. But as so many arrangements are united in the stirrup, its construction requires to be particularly explained. Plate XIV. gives three different views of the instrument, of the natural size, and as arranged for the 25lb. bar; the small and compound parts have been represented in a separate section, so as to exhibit their interior mechanism. It requires an attentive consideration on account of the many important parts compressed into so small a space at the stirrup: a clear comprehension of its mechanism will be obtained when we know the various concentric rotations which are performed at the stirrup,—the mode of checking and measuring these,—and their objects. The rotations are the following.

1. Of the mirror on its pivot;—the whole of the other portions of the instrument remaining unchanged.

2. Of the mirror, with its pivot and alidade, on the circle to which the suspension-screws of the wires are fixed, and on which the stirrup and its alidade rest.

3. Of the stirrup with its alidade, on the circle on which it rests.

In order to complete the view of all the rotations, we may here add, 4. That of the two upper extremities of the wire around one another, *i. e.* around the same axis as that on which the other rotations take place.

The *first* rotation will be sufficiently intelligible from figs. 1 and 3, Pl. XIV. The arrangement is simple, because its amount does not require to be measured. Its object is merely to allow of perfect freedom in fixing the theodolite; the axis of the mirror can always be made to revolve, to suit the position of the telescope and the scale, wherever they may be placed. The image of the scale which appears in the mirror serves itself to regulate the rotation, and no further arrangement for measuring it is required. A screw, as exhibited in the figs. 1 and 3, fixes the mirror in its position.

For the *second* rotation, the three pieces, the mirror, the pivot, and its alidade, are firmly connected as one piece, and revolve together in the cavity of the circle; they are represented, together with the latter, in a cross section, at fig. 4. The mirror is placed on the upper end of the pivot *B* at *A*; *C* is the alidade of the pivot; *D* is the circle. The only essential difference between the second rotation and the first is that in the second the angular amount can be measured. As the revolving alidade of the pivot, situated beneath the circle, embraces at its two extremities the edge of the circle, it forms on its upper and graduated surface two verniers, the inner margins of which lie close to the outer margin of the divided circle. A clamp, by which the alidade of the pivot can be pressed firmly against the circle, is seen in the section at *E*, fig. 4.

The second rotation alone would be sufficient if there were at no time an impediment to its use. The verniers on the alidade of the pivot come in certain cases beneath, and are hidden by the alidade of the stirrup. In the instrument represented in Pl. XIV. much care has been employed to restrict this within very narrow limits, as will be plainly perceived in fig. 2; but, in order to meet the rare cases in which it does still occur, without having to alter the position of the theodolite, both rotations may be em-

ployed at the same time, so as to free the indices without turning the mirror from the scale.

The *third* rotation is that of the stirrup with its alidade, on the circle upon which it rests. The directive force of the wires acts immediately on the circle to which the suspension screws are fixed: the directive force of magnetism acts immediately on the stirrup in which the magnet-bar is placed. When, therefore, these two directive forces form an angle with each other, the two parts upon which they act will have a tendency to move in opposite directions. That no such displacement of the parts may occur, they are made to slide on each other with so much friction, that the two directive forces, when forming a large angle with each other, may not be able to overcome it. For a similar reason it was provided in the unifilar that the alidade of the stirrup should be placed on the outermost margin of the circle, so that the friction produced by its pressure might act with the greatest leverage. The same has been done with the bifilar, where this provision is much more essential and important, the forces which tend to displace the two parts being much more powerful. Further, we must be able to measure with great exactness this rotation, on which depends the angle which the two directive forces form with each other. The simplicity of construction of the bifilar consists chiefly in this circumstance, that the same circle and graduation serve for measuring both the second and the third rotation. For this reason the alidade of the stirrup is also furnished with two noniuses. The instrument consists, therefore, of a circle with two alidades, which may be used independently of each other. In order that this independent use may never cause the two alidades to interfere, the one is situated beneath, and the other above the circle. But since each alidade is provided with two noniuses, and all four are to move on the divided limb of the circle, which is its upper surface, the inferior alidade embraces the margin of the circle and forms noniuses which abut at the outer margin, whilst those of the upper alidade, in order not to come in conflict with those of the lower, abut on the inner margin. The noniuses of the upper alidade can thus pass by those of the lower one, and even an interval may exist between them, which, however, must be smaller than the length of the divisions on the circle. Thus the graduation of the circle serves two purposes, the one not interfering with the other, only it cannot serve both purposes at the

same time, as the figures must be covered either by the noniuses of the one or of the other alidade, according as they are inside or outside of the graduation. For this reason the figures are placed alternately on the inner and on the outer side, as exhibited in Pl. XIV. fig. 2.

The *fourth* rotation is that of the two upper ends of the wires. No mechanical arrangement is required for this rotation; but the bearer on the ceiling, by which the wires are carried and adjusted, is turned by the hand. As the bearer must be fixed to the ceiling, no use is ordinarily made of this rotation; but it is so arranged in the first instance as to be in the most convenient position for all purposes. That position may be regarded as most convenient in which the lower ends of the wire interfere least with the mirror which is situated between them. It will be evident that, in the various uses to which this instrument is applied, if the bearer is not moved, the lower ends of the wire are brought into various positions, while the mirror retains its position between them nearly unchanged, being always directed towards the scale. The two wires, for instance, will sometimes be in one vertical plane throughout their whole length; sometimes they perform part of a revolution round each other, and a vertical plane drawn through them will form with the former one an angle, which is, however, always less than 90 degrees. If it be now so arranged that in the first case the plane of the wires coincides with the vertical plane of the optical axis of the telescope, the one wire passes just as far from the mirror in front as the other does behind, and both wires are as far as possible from the mirror. If the instrument is then arranged for the other use, the wires are brought nearer to the mirror, but not so as to touch it, even if the mirror were larger than the intervening space, because the rotation does not amount to 90°. It is always less than 90°, because the directive force arising from the suspension must be greater than the magnetic directive force; hence the moments of rotation arising from the two forces will only equilibrate when the wires undergo a smaller rotation than the magnetic axis; and since the latter, in the transverse, must be 90° from its natural position, it follows that the rotation of the wires must be less than 90°.

3. *On the use of the Bifilar Magnetometer.*

I shall in conclusion briefly notice the series of experiments

which must be performed in fixing and adjusting the apparatus.

1. The clock, the theodolite, and the scale are fixed, and a plumb-line dropped from the centre of the object-glass across the scale. The theodolite is to be leveled.

2. The telescope is directed to the opposite wall, on which there is a mark, serving to designate the terminal point of the optical axis. The scale is placed perpendicularly to the vertical plane of that axis.

3. A place is sought in the vertical plane of the optical axis for the mirror, the distances of which from the centre of the object-glass, and from the division of the scale across which the plumb-line is suspended, are, together, equal to the distance of the mark from the centre of the object-glass. The horizontal plane of this point must bisect the plumb-line from the centre of the object-glass. A plumb-line is let fall from the ceiling through this point.

4. The bearer is either fixed to the ceiling, or perpendicularly above a hole made through the ceiling, from 80 to 100 millimetres wide; so that the ends of a thread passed over it, and extended by small weights, pass freely through the aperture, and are both situated in the vertical plane of the optical axis of the telescope.

5. One end of a steel wire, sufficiently strong to carry half the weight of the instrument without danger of breaking, is fastened to one end of the thread, and drawn up to the bearer by drawing the other end of the thread down (care being taken that the wire and the thread should always be extended in a straight line); it is passed over the two cylinders of the bearer, and drawn down; the thread is then removed, and the two ends of the wire, weighted, are left to hang freely until they have assumed their natural position.

6. The two ends of the wire are cut off about 100 or 150 millimetres below the place where the magnetometer is to be suspended, and are fixed to the suspension screws. The stirrup thus carried is then, with the help of the screws, wound up into its proper position.

7. A box, sufficiently large to contain the magnet-bar, is placed underneath to protect the instrument in case the wires should break, and to prevent currents of air. This box is closed on all sides. Its lid consists of two halves, which fit close, and leave only *one* round aperture, through the centre of which the pivot

passes; the upper end of the pivot carries the mirror, which must be above the box. The two wires having the mirror between them pass through the same aperture. This circular aperture is usually closed by two semicircular flaps, in which there are small slits for the pivot and the wires.

8. Before the magnet bar is laid in the stirrup, a weight of the same size, but unmagnetic, is placed therein, and the wires are suffered to arrange themselves in their natural position, in which both are in one vertical plane throughout their whole length. The alidade of the stirrup is then brought as exactly as possible into the mean magnetic meridian from which the changes of variation are to be measured. The other alidade on the pivot of the mirror should be so fixed as to form a right angle with the alidade of the stirrup, in order that the noniuses may be far apart. The weight in the stirrup is moved until the mirror is situated exactly between the two wires, when the axis of the mirror should be very nearly horizontal. Employ the first rotation to direct the mirror towards the scale, without disturbing the alidade. If the scale does not appear in the telescope, it will be seen by the naked eye a little above or beneath, and may be brought into the field by the help of a light running weight placed on the stirrup. The first observation is then performed, and the position of the scale determined.

9. The time of vibration for determining the directive force of the wires may be observed before the magnet-bar is inserted, and again with a known increase of the moment of inertia. It is better, however, to perform this experiment somewhat later, when the distance of the wires from each other has been accurately adjusted, in case this distance has not been previously determined by calculation, and regulated accordingly.

10. The magnet-bar is then placed in the reverse position, (north towards the south) and the position of the scale again observed: this ought to agree with the observation (8.) If the two readings do not coincide, agreement must be attained by merely turning the stirrup with its alidade. The coincidence of the two readings proves that the magnetic axis of the bar is situated in the magnetic meridian. The less the directive force arising from the mode of suspension exceeds the magnetic directive force, (see 8.), the more delicate is this test, so that it may be impossible to obtain a *perfect* coincidence of the two readings; a difference of a few divisions of the scale may then be considered as unimportant. The influence of the hourly va-

riations must be attended to, by making continued observations with a second apparatus of the same kind, or by making continued observations of the time of vibration with a common magnetometer.

11. The time of vibration, t, is observed in this reverse position.

12. The magnet-bar is then placed in its natural position, (north towards the north,) by turning the stirrup with its alidade exactly 180 degrees; the time of vibration, τ, is again observed. Then the magnetic directive force, M, is to the directive force arising from the mode of suspension, S, in the ratio

$$M : S = t^2 - \tau^2 : t^2 + \tau^2.$$

When this proportion deviates much from unity, the wires must be brought nearer to or moved further from each other, until the altered directive force of the wires exceeds but little the magnetic directive force; for instance, by about the tenth part of the latter. This is the case in the Göttingen magnetometer.

13. Seek the angle z, the sine of which is

$$\sin z = \frac{t^2 - \tau^2}{t^2 + \tau^2}.$$

turn the alidade of the stirrup (say in the direction of the daily motion of the sun) $90° - z$, and turn the alidade of the pivot of the mirror in the opposite direction through the angle z. The equilibrium is then disturbed: the wires can no longer remain in their natural position, but must turn the circle to which they are fixed (and thus the whole instrument) exactly through the angle z, in the direction of the daily revolution of the sun. In this new position the equilibrium may be re-established, since the bar makes with its former position an angle $(90° - z) + z = 90°$, while the wires have only been turned through the angle z at their lower ends. It follows, thence, that if the wires were previously in their natural position, and if the magnetic axis of the bar was situated in the magnetic meridian, the opposite moments of rotation arising from the two forces M and S are to each other in the proportion

$$M \sin 90° : S \sin z.$$

But as

$$M : S = t^2 - \tau^2 : t^2 + \tau^2$$

$$\sin z = \frac{t^2 - \tau^2}{t^2 + \tau^2}$$

$$\sin 90° = 1.$$

The equality of these opposite moments of rotation, or the equilibrium of the instrument in this position, is the result. Whether the true position of equilibrium coincide with the calculated one or not, is proved immediately by an observation of the scale, which ought to be the same as before. For the mirror has been turned (together with the whole apparatus) the angle z in the direction of the daily motion of the sun; but having been turned by its independent motion the same angle z in the opposite direction, it consequently retains its first position, and the point of the scale is unchanged.

14. If, however, the observation shows an alteration of the scale, it follows that the supposition in the first experiment—of the magnetic axis of the bar being in the magnetic meridian—was not accurately fulfilled. The amount of the error can be calculated, and the experiments repeated. This calculation will be still more accurate and certain, if a corresponding experiment has been previously made, proceeding precisely as described in (13.), only making all the rotations in the contrary direction.

15. When the required coincidence has been obtained, the magnetometer remains in its transverse position. Its time of vibration is then, according to a simple theorem, the geometrical mean between the times of vibration t and τ, and the observations of changes of intensity can be arranged like those of the changes of declination. The changes of intensity are obtained in divisions of the scale. If we desire to convert them into fractions of the entire intensity, these are obtained by multiplying the arc value of the scalar divisions (expressed in parts of the radius) by

$$\cot z = \frac{2\,t\,\tau}{t^2 - \tau^2};$$

for the value in arc of the parts of the scale, expressed in parts of the radius, gives immediately the changes of intensity in parts of the directive force, which, under the prescribed conditions, is $S \cos z$. If we divide this directive force by the whole intensity, *i.e.*, by $S \sin z$, we obtain by multiplying the value of the arc by the quotient, $\cot z$—the changes of intensity in fractions of the whole intensity.

Article VIII.

Contributions to our Knowledge of Phytogenesis; by Dr. M. J. Schleiden*.

[From Müller's *Archiv für Anatomie und Physiologie*, Part II., 1838.]

The universal fundamental law of human reason, its undeviating tendency to unity in its acquirements, has from the first been evinced in the department of organized bodies as in all branches of science, and various attempts have been made to establish the analogies between the two great divisions, the animal and vegetable kingdoms. But although so many eminent men have devoted their attention to this subject, it cannot be denied, that all attempts hitherto made with this view must be considered as entirely unsuccessful. If, indeed, the fact has been of late generally admitted, still the reason of this circumstance has not always been correctly conceived and stated in its full clearness and precision. The cause, however, lies in this; that the idea of individual in the sense in which it occurs in the animal nature, cannot in the least be applied to the vegetable creation. At the most we can speak of an individual in its true sense only in some of the lowest orders of plants, in some Algæ and Fungi, which consist only of a single cell. But every plant developed to a somewhat higher degree, is an aggregate of fully individualized independent beings, even the very cells.

Each cell leads a double life: an entirely independent one, belonging to its own development alone; and an incidental one, in so far as it has become the constituent part of a plant. But it is easy to perceive that, as regards vegetable physiology as well as comparative anatomy in general, the vital process of the single cells must form the very first, absolutely indispensable fundamental base; and, therefore, at the very outset this question especially presents itself: *how does this peculiar small organism, the cell, originate?*

The great importance of this subject may, perhaps, be a sufficient excuse for my venturing at present to publish the following remarks, feeling as I do only too well that more extended

* Translated and communicated by Mr. William Francis.—The Editor is indebted to J. J. Bennett, Esq. for his assistance in revising the Translation.

observations can alone impart to them their proper scientific value. Perhaps, however, I may succeed by these remarks in drawing attention to this highly important subject.

Since no real advance in the acquisition of knowledge results from the attempt to explain processes in nature hypothetically, and least of all, where all requisites for a tenable hypothesis, namely all guiding facts, are wanting, I may omit all historical introduction; for as far as I am acquainted, no direct experiments exist at present on the origin of the cells of plants. That Sprengel's supposed primitive cells are solid granules of amylum, has long since been demonstrated. To enter into Raspail's observations seems to me to be inconsistent with the dignity of the science. He who feels any desire to do so may turn to the work itself.

The only researches connected with this subject, the highly important ones of De Mirbel, I shall have occasion to advert to subsequently, since even he does not make any mention of the progress of the formation of cells. It is to be regretted that Meyen, who, perhaps, has studied vegetable anatomy more extensively than any one up to the present day, has confined himself almost solely to the examination of developed forms, and has not yet brought the formative process itself in any degree into the field of his inquiries. I have still many doubts, the solution of which I had hoped to have found in his *Vegetable Physiology*, but found them not.

It was Robert Brown, who, with his natural genius and comprehensive power of mind, first conceived the importance of a phænomenon which, although observed previously by others, yet had been left totally unregarded. He found at first in the *Orchideæ*, in a great portion of their cells, chiefly in the epidermis, an opake spot designated by him *areola* or nucleus of the cell. He subsequently pursued this phænomenon in the earlier stages of the pollinic cells, in the young ovulum, in the stigmatic tissue, not merely in the *Orchideæ*, but also in many other Monocotyledons, and even in some Dicotyledons. It was natural that the constant presence of this *areola* in the cells of the very young embryo and in the newly originated albumen should strike me in my extensive researches respecting the development of the embryo; and thus, from the consideration of the various modes of its occurrence, the thought very naturally arose, that this nucleus of the cell must stand in a close re-

lation to the origin of the cell itself. I consequently directed my attention especially to this point, and was so fortunate as to see my endeavours crowned with success.

Before, however, proceeding to the communication of these observations, it is necessary that I should describe somewhat more at length the nucleus of the cell. As I have to treat of an entirely peculiar, and, as it appears to me, of an universal elementary organ of vegetables, I do not deem it necessary to excuse myself for applying to this body a definite name, and shall term it Cytoblast (κυτος βλαστος) with reference to its function, which will be subsequently described.

This formation varies in its outline from oval to circular, according as its solid form seems to pass from that of the lens into the perfect sphere. The oval and flat ones I have found most frequently in monocotyledonous plants, in the albumen, and in the pollen; the globular chiefly in the Dicotyledons, and in the leaf, stem, articulated hairs, and similar formations; however, no exclusive rule can be asserted in this respect.

The colour of the cytoblast is in general yellowish, yet sometimes passing almost into a silver white. I observed it to be most transparent in the albumen of some water plants, in the unripe pollen, in some *Orchideæ*, and also in the rudiments of the leaf in *Crassula portulacea*. It is scarcely to be distinguished, on account of its excessive transparency, in the sporidia of some Helvelloids. It is coloured by iodine, according to its various modifications, from a pale yellow to the deepest brown.

Its size varies considerably. It is in general largest in Monocotyledons, and in the albumen; smallest in Dicotyledons, in the leaf, stem, and their metamorphosed parts. The largest that I have seen was 0·0022 Prussian inch* in diameter (in *Fritillaria pyrenaica*); the smallest in the embryonal extremity of the pollinic cellule of *Linum pallescens*, from 0·00009 to 0·0001 Prussian inch. In the albumen of *Abies excelsa* I found it, on the average of several admeasurements of individuals of apparently equal size, from 0·00034—0·00059—0·00079. In the young leaves of *Crassula portulacea*=0·0003, and in the albumen of *Pimelea drupacea*=0·00095—0·001055. However, little importance can be attached on the whole to these measurements, as they increase and diminish; and it cannot be determined in what period of its life the cytoblast is examined. Its internal

* The Prussian inch is to the English as 1·03 to 1.—Ed.

structure is mostly granular, without however the granules of which it consists being clearly distinct from one another. Its consistence is very various, from such a softness that it almost dissolves in water, to that degree of firmness that it bears even the pressure of the compressorium without losing its form. The nearer it is to its origin the softer it is, and also where its existence is merely transitory. It is denser and more sharply defined where it goes through the whole vital process of the plant as a permanent tissue, as in the *Orchideæ*.

These peculiarities have been more or less completely described by R. Brown (Organs and Mode of Fecundation in *Orchideæ* and *Asclepiadeæ*; *Linn. Trans.* 1833*, p. 710) and recently by Meyen (*Physiology*, &c., vol. i. p. 207). But a circumstance has escaped these two most acute observers, which I nevertheless am inclined to place amongst the most essential. In very large beautifully developed cytoblasts, for instance in the recently originated albumen of *Phormium tenax* and *Chamædora Schiedeana* (Plate† XV. fig. 5), there is observed (whether sunk in the interior or on its surface was not evident to me) a small, sharply defined body, which, judging from the shadows, appears to represent a thick ring or a thick-walled hollow globule. In less developed individuals only the outer sharply defined circle of this ring can be observed, and in its centre a dark point, for instance in the stipes of the embryo of *Limnanthes Douglasii*, *Orchis latifolia* (Pl. XV. fig. 21), *Pimelea drupacea* (fig. 14, 15). In still smaller cytoblasts it appears merely as a sharply circumscribed spot; this is most frequently the case in the pollen of *Richardia æthiopica*, in the young embryo of *Linum pallescens*, and in almost all *Orchideæ* (fig. 16). Or lastly, there is observed only a remarkable small dark point. In the very smallest and most transitory cytoblasts (for instance in the leaves of Dicotyledons) I have hitherto not been able to discover it. In very rare cases, and those probably mere exceptions, and always only where the majority exhibited the simple nucleus, I have also found two; for instance in *Chamædora Schiedeana* (fig. 6, 7), *Secale cereale*, *Pimelea drupacea* (fig. 14):—in the two latter I have found sometimes even three (fig. 15). From my observations on all plants which admitted of a complete watching of

* Read at the Linnæan Society Nov. 1, 1831.

† The Editor has been favoured, through the kindness of Dr. Schleiden, with impressions from the copper-plates engraved under his superintendence. Having been printed abroad, the numbers have not been placed on the plates, but they are referred to in this work as Plates XV. and XVI.

the entire process of formation, it follows that these small bodies are formed earlier than the cytoblast (Plate XV. figs. 1 and 2), and I would almost suggest the conjecture that they are not entirely foreign to the nuclei shown by Fritzsche to exist in starch, and probably even identical with them. The size of this corpuscle also varies considerably, from the extent of half the diameter of the cytoblast to the most minute point, whose size did not allow of measurement, because it was even much exceeded by the thickness of the thread of the diaphragm of the microscope. In the albumen of *Abies excelsa* I found it to be on an average from 0·000045—0·000095 Prussian inch ; in *Pimelea drupacea*, from 0·00029—0·0003. Sometimes it appears darker, sometimes brighter than the remaining mass of the cytoblast. In general it has more consistency than the latter, and still continues sharply defined when this has been changed by pressure into an amorphous mucus, as for instance in *Pimelea drupacea.*

A second point, on which I must make a few observations so as to be able to express myself hereafter more briefly without being unintelligible, relates to the various inorganic substances which occur during the vital process of plants, and belong to the series of amylum and of the woody fibre. I do not at all pretend fully to enumerate in this place all substances chemically distinct; as little do I require that chemists should approve and adopt all my terms and characteristics (perfection independent of this would at present be an impracticable task): it is my intention merely to notice in a few words the most important modifications, their consequence and purport in the progress of the development of vegetable organization, in order to spare repetitions in future.

Starch, in the plant, appears to take the place of animal fat. It is superfluous nutritive substance, which is deposited for future use, and we consequently find it abundant in places where after a short repose a new formative process is to commence, or where a too luxuriant life has originated a superabundance of nutritive matter. It has of late been the subject of such deep research that it is not necessary to enter more fully upon this head: I shall merely refer the reader to the most recent and best summary of the results, in Meyen's *Vegetable Physiology*, vol. i. p. 190, &c. Frequently the place of the starch is occupied by a semi-granular substance, for instance in pollen, in the albu-

men of some plants, and frequently in the cells of the leaf as containers of the chlorophylle. It is chiefly distinguished by its occurrence in all kinds of granular forms without any interior structure, and from its being coloured by tincture of iodine, brownish-yellow, or brown. This substance, which I shall call mucus, is probably identical with that of which the cytoblast consists, and with the small granules in gum which I shall presently mention; the first conjecture Meyen (*Vegetable Physiology*, vol. i. p. 208) has noticed as being very probable.

Now when the starch is to be employed in new formations, it dissolves, in a manner as yet totally unknown to chemistry, into sugar or into gum, the latter appearing at times to pass into the former, or *vice versâ*. The sugar appears in the form of a perfectly transparent fluid, almost as clear as water, is not rendered turbid by alcohol, and takes from the tincture of iodine only a colour in proportion to the strength or weakness of the solution of the agent.

The gum appears as a somewhat yellow, more consistent, less transparent fluid, which is coagulated granularly by the tincture of iodine with a pale yellow permanent colour.

In the further progress of organization, in which the gum is always the last immediately precedent fluid, a quantity of exceedingly minute granules appear in it, most of which, on account of their minuteness, appear merely as black points. The fluid then seems to take from iodine a somewhat darker yellow, but the granules, when their size enables their colour to be distinguished, seem to become by this process of a dark brownish-yellow.

This is always the mass in which organization takes place, and the newly formed parts consist again principally of this separate transparent substance, which, on being subjected to pressure, presents to view an homogeneous colourless mass; when dried it imbibes water and swells; it is not at all affected by iodine, nor does it even imbibe it, but appears after pressure colourless as before, and so completely transparent, that, if not surrounded by coloured or opake bodies, it is totally invisible. This substance is of frequent occurrence in plants (for instance, in great quantity, together with a little starch, in peculiar large cells in the tubers of *Orchis*); I shall call it for shortness sake vegetable gelatin, and am inclined to enumerate under this head, as mere slight modifications, pectine, the basis of gum

tragacanth and many of those substances commonly arranged under vegetable mucus. It is this gelatin which is ultimately converted by new chemical changes into the actual cellular membrane, or its thickening layers, and into vegetable fibre.

I now return to the subject itself. There are two places in plants where the formation of new organization may be observed most easily and clearly, from their being cavities closed by a simple membrane, viz. in the large cell which subsequently contains the albumen of the seed, the embryonal sac, and at the end of the pollen tube, from which the embryo itself is developed. They are chiefly distinguished from each other by the embryonal sac, never originally containing starch, but probably, in general, the saccharine solution, (whence arises the sweet taste of unripe pod fruits and cerealia,) or gum.

The pollen on the contrary constantly contains starch, or the above-mentioned granular mucus representing it, as an essential constituent part. The so-called vegetable Spermatozoa will probably on more accurate examination be generally reduced to one of these substances. These substances, however, are soon dissolved, and change either into sugar or into gum; both, at times, even before the pollen grain has commenced sending forth tubes on the stigma, frequently in the progress of the descent of the tube through the style to the ovule; so that in some cases even unaltered starch is still found in the embryonal end.

At both these places the above-mentioned minute mucous granules very soon originate in the gum, upon which the solution of gum, hitherto homogeneous, becomes opalescent, or, through the presence of a larger mass of granules, even opake. Single, larger, more sharply defined granules (fig. 2 above) now become apparent in this mass; and very soon afterwards the cytoblasts occur (fig. 2 below,) appearing as it were like granular coagulations around the granules. The cytoblasts, however, in this free state grow very considerably; and I have observed, for instance in *Fritillaria pyrenaica*, a gradual expansion from 0·00084 to 0·001 Prussian inch.

As soon as the cytoblasts have attained their full size, a delicate, transparent vesicle rises upon their surface: this is the young cell, which at first represents a very flat segment of a sphere, whose plane side is formed by the cytoblast, and the convex side by the young cell, which is situated on it somewhat like a watch glass on a watch. In its natural medium, it is di-

stinguished almost by this circumstance alone, that the space between its convexity and the cytoblast is perfectly clear and transparent, and probably filled with an aqueous fluid, and is bounded by the surrounding mucous granules, pressed back by its expansion, as I have endeavoured to represent it in Plate XV. figs. 4, 5, and 6. But if these young cells are isolated, the mucous granules may almost entirely be removed by shaking the stage. They can however not be observed for any length of time, for they dissolve entirely after some minutes in distilled water, and only leave the cytoblasts behind. The vesicle gradually extends, and becomes more consistent (fig. 1 *b*.), and the covering now consists, with the exception of the cytoblast, which always forms one portion of the wall, of gelatine. The entire cell now gradually increases beyond the margin of the cytoblast, and quickly becomes so large, that at last the latter merely appears like a small body inclosed in one of the side walls. At the same time the young cell frequently exhibits highly irregular indentations (fig. 1 *c*.), a proof that the expansion does by no means proceed uniformly from one point. After further progressive growth of the cell, and evidently arising from the pressure of the neighbouring objects, the form becomes more regular, and then also frequently passes into the form, so beautifully determined *à priori* by Kieser, of the rhomboidal dodecahedron (compare fig. 1. from *b*—*e* and fig. 8.). The cytoblast is still found to be inclosed in the wall of the cell, at which place it passes through the whole vital process together with the cell formed by it, if it be not in cells destined to higher development, either reabsorbed at its place, or, after having been cast off as a useless member, dissolved in the cavity of the cell, and there reabsorbed. It is only after its absorption, that the formation of secondary depositions, as far as I was able to observe, commences on the inner surface of the sides of the cell. (fig. 9.)

In general it is rare that the cytoblast accompanies the cell which it produced through its entire vital process: nevertheless it is,

1. Characteristic of the families of *Orchideæ* and *Cacteæ* that in them a portion of their cellular tissue remains during the whole vital period in a lower stage of development;

2. It sometimes occurs in various plants, that the cellular tissue, which is merely of transitory import, is not perfectly developed, but retains the cytoblast, and is subsequently reab-

sorbed cotemporaneously with it. Yet I have also observed that the latter in the middle period of its existence lost much of its distinctness and sharpness of outline, which however reappeared when the reabsorption had commenced; for instance, in the nucleus of the ovules of *Abies excelsa, Tulipa sylvestris,* and *Daphne alpina.* It is inconceivable how some physiologists have been able to deny reabsorption in plants, since even very considerable portions of cellular tissue, for instance of the nucleus of the ovule, become wholly fluid again, and are received into the common mass of sap. This indeed only takes place so long as the cell still consists of the simple original membrane, and whilst it is not so far advanced in its individual development that its wall is thickened by secondary deposits.

3. In some rare cases the cytoblasts also remain persistent in the pollen granules; this is the case in some, perhaps all the *Abietinæ.* The lenticular cytoblast has already been observed by Fritzsche in *Larix europæa,* but its nature not understood.

4. Many hairs, especially the articulated and such as exhibit circulation of the sap in their cells, retain the cytoblasts (*c, f.* fig. 25.). It is remarkable, and moreover a proof of the close relation in which the cytoblast stands to the whole vital activity of the cell, that the small currents frequently covering the entire wall reticularly, always proceed from it and return to it, and that *in statu integro,* it is never situated without the current (fig. 25.).

The above described development of the cells I have observed in their whole course in the albumen of *Chamædorea Schiedeana, Phormium tenax, Fritillaria pyrenaica, Tulipa sylvestris, Elymus arenarius, Secale cereale, Leucoji* spec., *Abies excelsa, Larix europæa, Euphorbia pallida, Ricinus leucocarpa, Momordica elaterium,* and in the embryonal end of the pollen cell of *Linum pallescens, Œnothera crassipes,* and a number of other plants. It was only in the summer of 1837, after this memoir had been written, that I took up the examination of the *Leguminosæ,* and found to my surprise that in these plants, so frequently examined and everywhere employed as examples in the history of vegetable development, this process, overlooked by all observers, might most beautifully and easily be studied. But, indeed, the saccharine fluid contained in the embryonal sac had not been considered worth examining.

Without exactly following up the whole course of the formation of the cells, I found the cellular nuclei previous to the oc-

currence of cells floating loose in a great many plants. Finally, not a single example has occurred to me of newly originated cellular tissue, cambium excepted, in which the cytoblasts were wanting. I therefore think I am justified in supposing the process above described to be the general law of formation of the vegetable cellular tissue in Phanerogamia.

My observations are much more limited with regard to the Cryptogamia; nevertheless I found the cytoblasts in the sporidia of the Helvelloids, where however, on account of their great transparency, they are only perceptible with very high magnifying powers and with a considerable darkening of the field. In the large yellowish cells in the interior of the so-called anthers in *Chara vulgaris* I have observed them. In the sporules of *Marchantia polymorpha* I also noticed their development into cells, one of which, pressing the original parietes of the sporule forwards, forms the long capillary root (Plate XV. fig. 18—20.).

It is evident from the preceding that the cytoblast can never lie free in the interior of the cell, but is always inclosed in the cellular wall; and in fact, as far as observation will allow us to draw conclusions from such ticklish examinations, in such a manner that the wall of the cell splits into two laminæ, one of which passes interiorly, the other exteriorly, over the cytoblast. That on the inner side is in general the most delicate, and frequently only gelatinous, and is also reabsorbed at the same time with the cytoblast (fig. 8, 16, 21.). Sometimes, in preparing sections, they are ruptured and scattered over the glass, which might lead one to suppose that they were free. And probably they are subsequently, on incipient reabsorption, disengaged from their connexion with the cellular wall, and then a slight touch may suffice to disturb them from their position. The wall of the cell is frequently thickened in their neighbourhood, especially where they are rather globular, for instance in the pollen tube of species of *Orchis* which has become cellular (fig. 16 and 20).

Meyen, who should always be consulted in relation to anatomical subjects, has endeavoured in his *Physiologie*, vol. i. p. 45, &c., employing in a very ingenious way his beautiful observations on the relations of structure in developed cells, to establish the opinion that the cell is formed of spiral fibres intimately superposed. My direct observation, which may easily be repeated by every one, gives, it is true, quite a different mode of formation; nevertheless I must bring the facts related by Meyen

into connexion with my discovery, in order not to leave an apparent contradiction unresolved.

Meyen himself correctly observes, when treating of those spiral tubes whose very narrow fibres lie close upon one another, that an enveloping membrane could indeed not be observed, but that this by no means justified our concluding on its absence; for if the thickenings of the cellular walls, which are formed in most, perhaps in all cases, in spiral lines, in those places where they make their appearance early, even when the original cellular wall itself is *in statu nascentiæ* and soft, are connected firmly with this latter, and at the same time the simple coils of the spiral fibre lie perfectly close on one another, so that with our present microscopes no space between them remains perceptible,—it naturally follows that on rupturing the entire membrane (the so-called unrolling of the fibres) the rupture in the direction of the coils of the fibre must be so sharply defined that our instruments would not possibly be able to show the unevenesses. At the same time it should be well remembered, that the original cellular membrane, especially in long cells of hairs, frequently undergoes so great an expansion, that at last it would be infinitely thin, so that even the thinnest and apparently most simple cellular wall would not exclude the possibility of its being composed of the original membrane, and of the secondary deposit. Now if we set out from the spiral cells and vessels, the distant coils of which admit of no doubt as to the existence of an exterior enveloping membrane, and if we follow up the presence of this membrane through all the forms of the constantly approximating coils of the fibre until only the feebleness of our optical means prevents further direct observation, the law of sound analogy would require us to admit even here the presence of a similar membrane. But there is yet a more direct mode of proof, namely the observation of the history of the development. It is quite an essential law that each cell (laying aside for the present the cambium) must occur in the form of a minute vesicle, gradually expanding to the size in which we find it in the developed state. Moreover it is the constant result of an extensive examination of this formative process that a cell never evinces a trace of a spiral formation, either in its appearance or on rupture, previous to its complete growth, *i. e.* before it has reabsorbed the cytoblast. In all spiral cells, cells which exhibit se-

parate fibres, we find the full-grown cells in the commencement still perfectly simple in their walls. Thus, for instance, I have observed this to be the case in all aërial roots in their outer parchment-like layer*. Meyen discovered the spiral fibres in *Oncidium altissimum*, *Acropera Loddigesii*, *Vanda teretifolia*, hort. bot. Berol. (rectius *Brassavola cordata*), *Cyrtopodium speciosissimum*, *Aerides odorata*, *Epidendron elongatum*, *Cattleya Forbesii*, *Colax Harrisonii* and *Pothos crassinervia*. This is still more evident in the true cortical layer of these aërial roots, where I discovered in *Colax*, *Cyrtopodium*, and *Acropera* the far more beautifully developed and much broader spiral fibres. In quite young aërial roots not a trace of them can be found, and their formation belongs decidedly to a process of lignification.

We may further be convinced of the subsequent period of the occurrence of spiral fibre in the pericarp of the *Casuarinæ*, the cells of which previous to or shortly after impregnation evince not a trace of spiral formation. Meyen has treated these fibrous cells in the envelopes of many seeds in a somewhat stepfatherly way in his *Physiologie*, which is the more to be regretted, as these interesting and often highly beautiful formations promise many conclusions respecting the physiology of the life of the cell, especially if we should take occasion to investigate accurately the individual development of several of them. I may be permitted to make a few observations on this head.

Their occurrence is more extensive than is generally supposed. They occur in the hairs of the pericarp in some *Compositæ*, where they were found by Lessing in *Perdicium taraxaci* and *Senecio flaccidus*, and by myself in *Trichocline humilis* and *heterophylla*.

They occur in the epidermis in many *Labiatæ*, for instance in *Ziziphora*, *Ocymum*, in most *Salviæ*, e. g. *limbata*, *hispanica*, *Spielmanni*, &c., and lastly in *Horminum pyrenaicum*. My uncle

* Meyen called this, in his *Phytotomie*, p. 163, an outer cortical layer, which was situated on the true epidermis of the aërial roots. In recent times some doubts have been raised as to the correctness of this view. It may however be almost incontrovertibly proved that the cellular layer termed epidermis by Meyen possesses actual stomata, which, from their being covered, usually indeed occur only in a rudimental state, frequently manifest a complicated structure, although deviating only in appearance, as in *Aërides odorata*, but often likewise occur quite in the ordinary form and distinct, as in *Pothos crassinervia*. Moreover it was not Dutrochet, as it would seem from Meyen's *Physiologie*, p. 48, but Link who first drew attention to this layer.

Horkel discovered them in all these many years ago; Baxter only noticed and published their occurrence in *Salvia verbenacea*. I can add to these *Dracocephalum Moldavica*.

R. Brown discovered them in the parenchyma of the pericarp in the *Casuarinæ*; and I have met with them in the spongy inflated cellular tissue of *Picridium vulgare*, occurring generally in a reticular form, and presenting an exquisitely beautiful appearance.

Horkel also discovered them in the epidermis of the seed itself in the *Polemoniaceæ* long before Lindley made known their presence in *Collomia linearis*. They occur in *Collomia, Gilia, Ipomopsis, Polemonium, Cantua, Caldasia*, and perhaps in the entire family, with the exception of *Plox*, to which genus *Leptosiphon*, in which are the first indications of them, is closely allied. Horkel had also studied them on the seeds of *Hydrocharis*, where they occur in the highest state of development, long before Nees von Esenbeck published this fact. Rob. Brown makes mention of them in the *Orchideæ*, which statement I find confirmed as to most of our native species of *Orchis*. Moreover I have discovered very beautiful spiral fibrous cells in the epidermis of the seed of *Momordica elaterium*, and a more reticular fibrous formation in *Linaria vulgaris, Datura Stramonium*, in *Salviæ* and in several other *Labiatæ*; probably it is common to the whole family.

Lastly, they occur, according to Horkel's discovery, in the parenchyma of the seminal integuments in *Cassyta* and *Punica*.

Whether these formations be studied in their individual development in a single species, or in their progressive stages in a series of allied plants, highly interesting general results will be found in both ways. The general and essential fact at which we first arrive is, that the fibres are never formed free, but in the interior of the cells; and that the walls of these cells in the young state are simple, and generally very delicate. M. Corda's statement respecting spiral cells without enveloping membrane (*Ueber Spiralfaserzellen*, &c., p. 7 and 8) is founded merely on inaccurate observation.

These cells in the commencement are usually filled with starch, rarely with mucus or gum. The starch always passes into the latter state, in the progress of development; and this is converted into gelatin, and, as it seems, gradually from the exterior towards the interior. This gelatin finally passes at its outer

surface into vegetable fibre, following the direction of a spiral line, the coils of which are sometimes narrower, sometimes wider. If these forms be observed in their successive stages of development and in their various conditions, the idea involuntarily forces itself upon one that the spiral formation arises from a spiral movement of a fluid on the walls of the cells between them and the central gelatin. Horkel has once actually observed, in *Hydrocharis*, the motion of small globules between the coils of the fibre whilst in the act of forming.

The highly varied appearance of the fibres seems to depend chiefly upon the time of their origin, and on modifications in the chemical changes of the formative substance. It probably depends solely upon the first circumstance, whether the spiral fibre lies free in the cell when it is formed very late, or whether it is adhering to the membrane of the cell, if its origin happens at a period when the cellular membrane itself is still very soft and gelatinous, and consequently can glue itself to the fibre, likewise still in a gelatinous state. This is the case in *Camarina, Cassytha, Hydrocharis, Trichocline, Orchis*, &c., but in general the wall of the cell is too far advanced to unite with the fibre, and it then lies loose in the interior of the cell. In this case the material is rarely consumed entirely in the formation of the fibre (although it always is when the fibre coheres with the wall) *e. g.* in *Salvia Spielmanni, Mormordica elaterium.* I have reason to suppose that this complete consumption almost always takes place, especially in spiral vessels, and is the cause of their conveying only air. More frequently, however, one or more fibres are formed; but then a great portion of the gelatine has still remained unconsumed, which, on moistening the cell with water, oozes out in a vermicular form, and in swelling expands itself over the fibres, thus appearing to surround them; this is the case in most *Salviæ* and *Polemoniaceæ*, in *Senecio flaccidus, Ocymum polystachyum* and *polycladum* (*Lumnitzera*, Jacq.). There is an intermediate form between this and the former when the gelatine itself forms a broad spirally wound band, which appears to be composed at its surface of innumerable delicate fibres; their occurrence in this state is very beautifully seen in *Perdicium Taraxaci* and *Ziziphora.* A much less advanced formation exhibits merely a thread or a cone of gelatine in the interior of the cell, the surface of which, however, is covered with delicate spiral lines. This occurs in some *Salviæ*, for instance in *S. verticillata*,

and in *Leptosiphon androsaceum*. Lastly, the lowest stage of development is that where the gelatinous thread, which is furnished with spiral striæ, has a hollow cavity in its interior, which still contains undecomposed starch; this instructive appearance is found in *Dracocephalum Moldavica*, *Ocymum basilicum*, and some allied species. In illustration of the above, consult Plate XVI. figs. 26–35, with their explanations.

Before I quit the spiral fibre, I will merely add, what indeed has been of late admitted by every good observer, that the only difference between spiral cell and spiral vessel consists in the dimensions; although constant transitions between them may be observed quite as well as between the liber and parenchymatous cells; and consequently, as regards the doctrine of this subject at least, there is no longer any place for natural philosophical phantasies of rigid images of higher types, and such like empty words. That which forms a cell of the liber out of a round cell, the preponderating expansion of an organ lengthwise, is also that which converts the spiral cells (the vermicular body) into spiral vessels. But the function of the spiral fibre is, as every honest vegetable physiologist will certainly admit, entirely unknown to us at the present day. It is certain that spiral vessels and spiral cells occur in the living plant quite as frequently filled with sap (in the young vegetating portions) as with air (in the older organs which have attained their full dimensions); and it is this which has given rise to the conflicting views of authors. But the same also occurs in all cells under certain circumstances; and the influence of spiral fibre remains totally in the dark and unexplained. Perhaps it may seem probable from the preceding that the spiral is everywhere only a secondary variation in form in the product of the vital principle (the fibrous substance) produced by a different tendency of the vital activity of the cell, as soon as this is forced, at a certain stage of its development, to give up its independent individuality, and to enter as an integral portion into the complexity of the entire plant.

Moreover I believe we may venture in conclusion to deduce from the data above enumerated, that this indication of spiral formation is the surest sign that we have no longer anything to do with the simple cellular membrane.

I now return, after this somewhat lengthy digression, to my subject. The process of the formation of cells, which I have endeavoured to explain at full, is in effect that which I have ob-

served in most of the plants which I have examined. There are however several modifications of this process which add in many parts to the difficulty of observation, nay sometimes render it quite impossible, although notwithstanding this the law remains in general indisputably valid, because the analogy requires it, and moreover we can sufficiently account for the reasons of the impossibility of direct observation.

The difficulties which I here notice arise especially from the physical and chemical properties of the substances preceding the formation of cells. The above enumerated ingredients are to be considered as scarcely anything else than some few points, which for the purpose of giving a general view, and to render the classification more easy, I have intentionally selected from the organic chemical processes of vegetable life, which are constantly in operation, and with which as yet we are entirely unacquainted. Almost all those substances exist constantly together in the living plant, and only their greater or less preponderance authorizes the expression, that the cell contains amylum or gum, and so forth. Towards the termination only of the individual life of the single cells do we find them filled with a less number of different substances; with one only, probably in those cells alone which contain volatile oil.

If now we suppose that the cell is entirely filled with a limpid solution of sugar in which gum is rapidly generated, but only just so much as is necessary to form, by as quick a conversion into gelatine, a delicate cellular membrane, whose existence, in consequence of a similar refracting power of the wall, of the contents, and of the surrounding medium, we are not able to distinguish with the microscope;—then it becomes highly probable, that a number of such formative processes may be going forward which escape our observation, and become known to us only by their results, when we find, after the reabsorption of the primitive cell, two new ones suddenly in its place. If on the contrary our attention has been previously directed to this process, we have, it is true, by employing reagents, especially iodine, which is quite indispensable to the physiological botanist, several means at hand of rendering it visible where such a formative process is suspected. Gradual transitions to the perfectly invisible processes will be easily found by extensive examinations: I will as an example just mention one of the most difficult cases I have met with. This occurs in the germination of the spores of *Marchantia poly-*

morpha. Of the cellular nuclei evident in the spores only few, in general only from 2 to 4, serve for the formation of the cells; the others are quickly enveloped in chlorophylle, and thus withdrawn from the vital process. The transparent liquid in which these cytoblasts float, passes through the other stages of the metamorphosis into cellular membrane just at the boundary of this latter, and so rapidly, that the excessively delicate young cells are distinguishable by nothing else than a fine, in general more or less uninterrupted circle of infinitely small, black granules, and by a scarcely perceptible greater transparency of the contents of the newly formed cells in comparison with that of the primitive cell; and, finally, under the most favourable circumstances by the place where the newly originated cells come in contact, and when this juncture is still covered by the membrane of the primitive cell. (Pl. XV., fig. 18—20.) In the Cryptogamia, and especially in water plants, this may perhaps be general; and probably Mohl's division of the cells of Confervæ might be thus explained.

If we consider, however, that there are undoubtedly many plants, among which should probably be reckoned more especially the Fungi and Infusorial Algæ, in which we are totally unacquainted, as yet at least, with the cytoblast, on account of its absolute minuteness and transparency; if further we bear in mind that the nucleus in the cell-germ, even in larger cytoblasts, appears frequently immeasurably small, or even with the highest magnifying power, still entirely escapes the eye; and, lastly, if we deduce from what has been previously stated, that nevertheless this granule, which can no longer be rendered perceptible, probably affords in the proper medium a sufficient cause for the formation of cytoblasts with which the whole formative process of the cells originates; then indeed we are forced to confess that imagination here obtains ample space to explain in every case the origin of infusorial vegetable forms even without the aid of a *deus ex machina* (the *generatio spontanea*). But it is my intention to communicate only facts and their immediate consequences, and not to dream; and I will therefore rather add a few more observations on the growth of the plant.

What is to grow? is a question which every child quickly answers in the expression, "When I am as big as father." There is truth in this answer, but this little will not satisfy science. Words have no value of themselves, but are like coin, only signs

of a value not exhibited in specie, in order to facilitate commerce. And to carry the comparison further, there follows an insecurity in this intellectual property, and frequently bankruptcy, if this coinage has not its unchangeable, accurately determined standard: in a word, the utility of a scientific expression depends on the accurate definition of the idea upon which it is based. Unfortunately the perversity of our social relations has made us entirely forget the original meaning of money; the sign has become to us the thing itself: may some good genius preserve us in our intellectual life from similar mistakes! We must here guard against two dangerous rocks; first, when words are transferred from one science to another without accurately testing whether they fit in their new place as to all their accompanying meanings also; and secondly, when we lose sight of the signification of a word consecrated by the spirit of the language and its historical development, and employ it without any further ceremony in compounds, where perhaps, at most, only an unessential part of its signification suits.

Thus, for instance, E. Meyer (*Linnæa*, vol. vii. p. 454.), after repeating the well-known experiments of Duhamel, lays down this position: "the law of the longitudinal growth of the internodes is, to grow *inter se*, or from above downwards." This position he requires for his theory, and consequently he must defend it in all ways, although he himself confesses that this reverse growth must appear to every one of his readers contrary to good sense. He would never have arrived at this position if he had more accurately analysed the word "grow," (to which he was accustomed in animal physiology,) in reference to its applicability to the plant: he would soon have found that the origin of new cells, and consequently the actual growth of the plant, constantly takes place in its outer portions upwards, and that his very comparison of the building up of a voltaic pile is exceedingly well adapted to refute himself. Nothing further would result from the experiments of Duhamel and Meyer, than that the inferior, *i. e.* precisely the first originated, older cells of the internode possess a greater power of extending in the longitudinal direction, and retain this capability longer than the younger cells.

With respect to the second point, we find an admirable example in the position frequently expressed of late, that the stem of the plant is formed of the *cohering* petioles. The word "cohere"

(*verwachsen*, to grow together) has possessed however from time immemorial, both in common life and in science, the signification that two or several originally and naturally separate parts have become by the process of growth either abnormally, or, under certain circumstances regularly, united. If, therefore, we apply the word "cohere" (*verwachsen*) to the stem of the plant, an organ, which, in every period of its existence, under all forms of its appearance, is a simple and undivided one, and at the origin of the plant even constantly makes its appearance earlier than the leaves with their petioles, there certainly is in this a monstrous misuse of language, and science itself can gain nothing by it, and even loses in the eye of the intelligent layman who sees through such a play upon words. What would the zoologist say were we to regard the trunk as a cohesion of the extremities?

But I come back to my question: What is To grow? An old twaddler says, To grow signifies increase of the mass of an individual, and takes place in the inorganic world by juxtaposition, in the organic by intus-susception. Have we gained anything by this for vegetable physiology? I think not. If the plant is to grow by intus-susception, then I say the plant consists of an aggregate of single, independent, organic molecules, the cells; it increases its mass by new cells being deposited on those already existing; consequently by juxtaposition. But the single cells in their expansion, frequently to an enormous bulk in comparison with their original size (I need merely call to mind the pollen tubes), also increase in substance in the interior of their membrane, and in this way also the mass of the whole plant is increased; it consequently grows by intus-susception also. Lastly, the cell deposits after a certain time new organic matter in layers upon its primitive membrane, therefore a juxtaposition again, which still however belongs to the cycle of the life of the plant. It is hence easily apparent that the idea "grow" still requires for the purposes of scientific botany a new foundation in order to be capable of being applied with certainty.

Of the three above-named cases, the second and third belong more to the individual life of the cells, and are of secondary importance only, as concerns the idea of the whole plant, regarded as an organism composed of a certain (1 to ∞) number of cells. The plant considered in its totality increases its mass, that is, the number of the cells composing it, in the first way only.

We must therefore discriminate here three processes essentially distinct from each other, which accurately considered scarcely find their analogue in the other kingdoms of nature.

1. The plant grows, *i. e.* it forms the number of cells it is to have.

2. The plant unfolds itself by the expansion and development of the cells that are formed.—It is this phænomenon especially, altogether peculiar to plants, which, because it results from their composition of cells, can never in any form, not even a remote one, occur in crystals or in animals.

3. The walls of the full-grown cells are thickened by fresh-deposited layers;—a process which, according to the old rule, *a potiori fit denominatio*, may be most properly termed the lignification of the plant.

If, with regard to the growth of the plant, we keep at present to the meaning of the word given under No. 1, then this question will arise—Where are the new cells formed? Three cases here comprise all possible answers: namely, the new cells are either formed outside on the surface of the entire previous mass; or in its interior; and then again either in the intercellular passages or in the cells themselves; *quartum non datur*.

Mirbel has, in two excellent and profound memoirs on the *Marchantia polymorpha*, which he presented to the French Academy in 1831 and 1832 (p. 32), proposed the idea, that all the three cases just mentioned as possible do actually occur in plants. Without meaning here to anticipate what follows, I must yet remark, that only one case (the formation of new cells in old ones) appears to be proved by his direct observations. The second case is merely a conclusion assumed; and lastly, the germination of the spores of the *Marchantiæ*, which was to explain the third case, has been observed by me to be quite different, as I have already represented above.

Lastly, however, we must still examine whether the difference of organs establishes a physiological difference of growth which deserves our attention. We may distinguish here four cases. We observe: 1. The development of the plant upwards (*in puncto vegetationis*, C. Fr. Wolff). 2. The elongation downwards. We thus comprise the formation of the necessary organs of the plant, of the stem, of the leaves, with their metamorphoses, and of the root. 3. We have to keep in view the production of the accidental organs, e. g. bulbs, &c. And 4.

We find an annual thickening of the axile formations, the development of the woody stem.

Let us now see which of the three possibilities of the formation of new cells, in each of the cases just enumerated, is actually realized. I have shown how the new cells are developed in the embryonal sac, and consequently in a large cell. A similar process is evident in the embryonal end of the pollen tube, consequently in a highly elongated cell; and I shall now proceed to delineate the further development of the embryo. After the first cells, generally few in number, have formed, they rapidly expand so much that they fill the pollen tube, which is then very soon no longer recognisable as the old enveloping membrane. But at the same time several cytoblasts again originate in the interior of each of these cells, and produce new cells, on the rapid expansion of which, the mother-cells also cease to be apparent and are reabsorbed. The same process is repeated indefinitely. But since the newly originated cells have continually less room to expand, and therefore constantly become smaller, the previous transparency is soon destroyed by the cytoblasts which are constantly being produced anew in the interior, and by the tissue becoming more and more compressed; and from this stage to the perfect completion of the embryo we are conducted by the clearly logical inference that the process thus introduced continues the same, since no new force comes into action, which might determine us to admit a sudden variation of the vital action, more especially as we very soon meet with the same indication of the vegetative power again.

The seed, meanwhile, germinates, and the embryo becomes a plant; and then indeed the question may arise, Does the process of life continue the same thenceforward, in the internodes and foliaceous organs? Now we are here very soon convinced of the negative,—that an origin of new cells on the surface of already existing organs does not take place. The surface is always smooth, and generally provided in a very early state with a kind of epidermis, the outer layer being more transparent and almost as clear as water; and never do we find even an indication of a newly formed cell on the surface.

But if the embryo is the image of the whole plant, and this latter does not present anything that is not a repetition of its organs, if we have found in the embryo that its growth only consists in the formation of cells within cells, we may expect to

find the same result also in the process of the growth of the entire plant. It is principally a foliaceous organ, the anther, which has hitherto been studied and followed in its development by many celebrated men (particularly well by Mirbel); and here it is quite decided that the increase of cells takes place within the old ones. And in this case the formative process certainly coincides with that above described. R. Brown and Meyen have enumerated many cases where they had observed the cytoblast in very young cells of the pollen. In *Pinus*, *Abies*, *Podostemon*, *Lupinus* and others, I have followed up completely the development of pollen after Mirbel; in *Abies* I have decidedly observed the cellular nuclei and their development into new cells within one another; and I have never missed the cytoblast in young cells.

Now if the pollen grains are nothing more than converted leaf-parenchyma, if the anther is merely a metamorphosis of the leaf, we may undoubtedly infer inversely, that the process which we have observed in it, and which characterized the formation of the embryo and cotyledons (as prototypes of the leaf), will be again found in all foliaceous organs. For the same reason which was stated in reference to the later stages of the development of the embryo, actual observation is infinitely difficult in this case. With a view to this I have nevertheless examined a large number of buds, and have convinced myself in the most decided way of the identity of the process both in the constantly elongating apex of the axis and in the leaves, which always originate somewhat beneath it. The best adapted for this purpose are the succulent plants, the *Aloineæ* and *Crassulaceæ*. *Crassula portulacea* seemed to me most advantageous, and in this I first succeeded in separating from their connexion some cells, in whose interior young cells were already developed, without however entirely filling the original cell. But having once become familiar with the subject, I was subsequently able to detect in all other plants these individualities from among the apparently merely semi-organized chaos. Another additional circumstance here indeed presents itself, which renders the subject much more difficult than with the embryo. For, setting aside the smallness of the cells, their walls, in the new-forming vegetable parts, still consist only of gelatine, and are so delicate, that it is exceedingly difficult to separate the parts intended for examination without destroying the organization altogether. (Compare Plate XV. figs. 22—24.)

This process is more easily discernible in articulated hairs, and such as have a head consisting of several cells, where the same appearances, which I have so frequently observed in the young embryo, and such as Mirbel has so beautifully described in the development of the gemmæ in the cups of *Marchantia*, may be easily and beautifully seen, for instance in the common potatoe. Meyen also has published similar observations, although he still expresses himself with some doubt. (Wiegmann's *Archiv*, 1837, vol. ii. p. 22.)

It is not until after as many cells are formed as the organ requires for its completion that the walls of the cells become firmer; and then commences the development of the organ by the mere expansion of the cells already formed.

But I must here enter somewhat more into detail, in order to explain the probable origin of the vascular bundles, and of the epidermis. At an early period a stripe of more transparent cells is defined in the axis of the leaf which is in the act of forming, in which no more new cells are developed, and these cells soon considerably exceed in size the cells of the remaining mass, which are constantly becoming smaller by continual division. These cells are the foundation of the future vascular bundle which forms the midrib of the leaf. For while the parenchymatous cells subsequently expand on all sides, these cells are only developed in their longitudinal dimension, and are thus able, although fewer in number, to follow the expansion of the other cells in the longitudinal direction of the leaf. It is not till a later period that these cells, by a difference of the internal depositions, separate themselves into spiral vessels and cells of the liber. The spiral vessels begin to be visible in the newly-formed parts, and also in the entire bud, always in the immediate vicinity of old, already formed spiral vessels; and they proceed in this manner away from the stem into the new parts. I do not understand therefore what is meant when the fibres of the stem are regarded as proceeding from the buds; one might quite as well consider the river as running from the ocean to its source.

A similar process takes place in the development of the side nerves of leaves. The formation of new cells generally ceases quite early in the outermost layer of cells. The cells are soon filled with a limpid fluid, and naturally become, on the expansion of the subjacent parenchyma, superficial, flat, and expanded.

The cells of the vascular bundle and of the epidermis appear in this way to be less potentialized [*minder potenzirt*]—are as

it were cells of lower dignity than the parenchymatous cells; and perhaps this physiological peculiarity is connected with the fact, that they more rarely secrete peculiar chemical substances, but are mostly thickened only by depositions within their walls of new vegetable fibrous substance, or more correctly, membranous substance. I cannot omit here venturing to throw out some hints, which perhaps are less intimately connected with the purpose of this Memoir, but which may probably at some future time be of importance, for the understanding of the entire plant. Let us once more pass under review the process of growth of the plant just depicted. A simple cell, the pollen-tube, is its first foundation. In this originate cells; in them are formed new cells, and so forth through the entire life. But here, the mode just mentioned of the origin of the vascular bundles and of the epidermis in relation to the parenchyma, would point to this fact; that the lower the dignity of the cell, 1. the greater power does it possess of expanding and of extending in length, and 2. the less capacity does it possess of forming peculiar fine substances in its interior. If now the potentialization of the cells goes on throughout the entire growth of the plant, there follows from thence a constantly closer approximation of organs otherwise kept asunder, and a constantly higher ennobling of the substances developed in the cells. Consequently, the lower parts of the internodes will appear to be more elongated than the upper; the leaves and young shoots (*summitates herbarum*, Pharmacol.) contain nobler saps than the stem; the members are shortened as they approach nearer to the upper terminal point of the plant, the leaves come closer together, and the result of this internal higher potentialization of the cell, of the constantly diminished expansion in length, of the constantly nearer approximation of the lateral organs, of the constantly more nobly developed substances, is, last of all, the flower, in its exclusive individuality, with its splendour of colour, its perfume, and its secret capacity of determining, by means of its juices, a single cell which is to develope itself anew into an independent plant and pass anew through the same cycle.

I return after this digression to my subject. Hitherto I believe I have demonstrated conclusively enough and in accordance with nature, that in the whole growth of the plant* cells are constantly formed only within cells. Let us now proceed to the

* I would observe, that in the whole Memoir in general only phænogamous plants are intended.

root. Here I can contribute but little to the explanation of the subject; for as yet I have not succeeded, in the rather limited researches that I have instituted, in coming to any satisfactory result; for I found it quite impossible to decide *the* question, whether there is secreted at the extremity of the radicle a liquid in which new cells are formed. On the other hand, it is certain that there exists in the extremity of the root a concavo-convex group (a meniscus) of cellular tissue in which the process of the formation of cells takes place in the same way as in the ascending parts of the plant. A main cause of the elongation of the root consequently consists in this,—that on the convex side of that cellular mass new cells are constantly formed in the interior of those already present, while on the concave side the cells already formed expand cotemporaneously, and generally indeed most predominantly in the longitudinal direction, and thus constantly push the extremity of the root before them.

The third case, the formation of the accidental organs of the plant, I must here entirely pass over, as I am wholly unfurnished with any personal observations on the subject. Probably, however, the process here is the same as in former cases, for Meyen (*Physiology*, Vol. i. p. 209,) observed the cellular nuclei in the germinating tubers of *Orchideæ*. Moreover, analogy leads to the same result, since all these parts are nothing more than morphological modifications of organs which have already been previously treated of. It still remains, however, for me to mention a fourth point, namely, the increase in thickness of plants forming woody stems (Dicotyledons). The origin and import of cambium is the nut upon which so many young phytologists have already broken their milk-teeth, the Gordian knot which so many botanical Alexanders have cut instead of untying, and the enigma in the solution of which almost all the Coryphæi of our Science have laboured with more or less success. My inquiries respecting this layer of distinct origin between bark and wood are by no means concluded.

Before I proceed, however, to the communication of my observations on this subject, it is requisite once more to take up the question of the individuality of plants.—I have above observed, that in the strictest sense of the word, only the simple cell deserves to be called an individual. If we go a step further, we might regard each axis with its lateral organs as simple beings. If, however, we disregard this composition of the plant of cells

and similar axes, and conceive as an individual in the organic world, that body which cannot, without losing its idea of totality, be divided into two or several, and whose vital process has a fixed point of beginning and ending in definite periodicity, it thence follows that only the herbaceous (*planta annua*) and the true biennial plants, which flower in the second year and then die off entirely, can be considered as individuals in the vegetable kingdom. The idea of individual life necessarily requires for a character individual death as a condition of the organization itself. But where such a death does not exist as a final termination from internal necessity, as an internal preconditioned cessation of the organizing force, there individuality must be out of the question. But this is only the case in the above-mentioned plants; and, consequently, from them solely must we set out, as from the prototype, in all inquiries regarding the nature and life of the vegetable organism.

To prepare for a transition to what follows, I shall turn to the exposition of the two different modes of propagation. It either takes place by a process which has hitherto been termed in plants impregnation, and to which has been ascribed a sexual difference, (Wiegmann's *Archiv*, 1837, Vol. i. p. 200, &c.) or by division, the plant, for instance, developing on itself a perfectly similar individual, and then at a certain time dismissing it. This latter, the formation of so-called bulbilli, &c. occurs together with the former only in a small number of plants. We must however make ourselves better acquainted with it. This creation, for instance, does not take place always in such a way as that the mother plant separates itself entirely from them, and scatters them singly; but it forms most frequently, before its individual death, a peculiar organ, which places the offspring in a peculiar vital connexion with one another, and at the same time serves as a reservoir for a certain quantity of nutritive substance, by which the first development of the young individuals is facilitated. But in general this organ is merely a metamorphosis of some other single well-known one, the stem or the root, or, as in the potatoe, the axillary buds; and consequently, in this case, no one has ever hesitated to speak of these things as of mere *parts of a plant*, which continue to live as connecting members between the younger *individuals* after the death of the parent. A different course on the contrary has been taken, when stem and root cotemporaneously, and therefore nearly

the entire totality of the plant, take part in this formation; and although the result in this case may probably be that there can be no question of an heteromorphy of a known part of a plant, yet the physiological identity in signification of this and the former case has not been steadily maintained, and the view has thus been obscured.

Most botanical writers set out quite at their ease, as if it were self-evident, from the tree as the perfect plant, and I believe it is not difficult to demonstrate that where Vegetable Physiology lies very deep in error this very misconception is solely to blame. Two quite distinct ideas have here been confounded, viz. the highest stage of development to which vegetable life can in fact raise itself, and the type upon which the idea of individual must be based. Now if the first of these ideas may be truly maintained with regard to the tree, yet the application of the second to it is in every respect totally false, as has been very correctly asserted before by M. Meyer (*Linnæa*, vii. p. 424). It necessarily belongs to the idea of a plant that it produces on its stem foliaceous organs; yet there is no tree that has leaves. Paradoxical as this may sound, yet it is not the less true. It is a fact that certainly no botanist is ignorant of, that no lignified part of a plant, even though only in its second year, is capable of producing a leaf; but the direct consequence is by no means so generally acknowledged, that for that very reason the woody stem cannot come under the idea of plant. From the error of regarding the tree as a single plant much confusion has arisen in our physiology, the definitions of the ideas of root, stem, bud, &c. have become very unsettled, and bitter controversies have been carried on respecting the functions of these parts, which could have no result, because the one party spoke of this, the other of that, this one of stalk, the other of stem, this of root-fibrils, that of ligneous root-substance.

But the so-called lignified root is just as little a root as the lignified stem is a stalk; but both together are, according to the idea, inseparable, and they form, moreover, altogether a purely accidental organ in the plant, which the annual individual has secreted on its surface, in order to bring into connexion, by means of a single organized membrane, the whole sum of new and young individuals. The tree corresponds entirely to the polypidom, and it appears to me not more sound to set out from it as the type in plants, than were the zoologist to set up a *Gorgonia* as the idea

of animal individuality. And this analogy is not in the least weakened by the circumstance, that exactly in the highest developed plants we meet with this woody stem most frequently; but it is on the contrary natural, that, if the animal kingdom receives in a certain measure its vegetative side from the vegetable kingdom, it should connect itself through the lowest stage of animals to the highest plants, while this vegetative half of the vital phænomena in the higher animals is in like manner illustrated and ennobled by the constantly more surely and more obviously independent individuality.

With this explanation of the woody stem (the root included), it will appear henceforward by no means remarkable that this organ (as if it were a mere organized groundwork) can produce upon every part of its surface young vegetable individuals, *i. e.* buds, as soon as it is in a condition to convey nutritive substance to these buds from any part, whether it correspond apparently to the former root or to the stem; while this purified idea of the plant leads to the law, that in the regular course of vegetation, neither root nor internode, but only the axilla of the leaf, is capable of generating a bud, *i. e.* a new axis with lateral organs.

But the following remarks, which in nature (who never, like a bad artist without a plan, fluctuates between the most opposite methods,) would be in the usual way of treating it an inexplicable contradiction and an absolute miracle, will serve for the decided establishment of this view.

We miss quite suddenly, for instance, upon the secretion of this organized mass, the wood, the influence of the law of formation, which, till then, had without exception, presided over the growth of the entire plant in all its parts. There are here formed, so far as we are yet acquainted with the subject, no cells within cells; there occurs here no expansion on all sides of the primitively minute vesicle; there is here no cytoblast from which the young cell might be developed—but under the outermost layers of cells which are comprised in the term bark, an organizable fluid pours itself, as it were, into a single large intercellular space, which fluid, as it appears, very suddenly consolidates in its whole extent into a new, peculiarly formed tissue of cells deposited on one another, the so-called prosenchyma. Here, moreover, decidedly no vascular bundles are formed from cells of lower dignity; for all the cells are cotemporaneous, and originate at their full size; and what has been called "spiral

vessels of the wood" is something immensely different from the spiral vessels of herbaceous plants, both with respect to their origin and probably also with respect to their physiological destination. In the controversies carried on, sometimes with great warmth, respecting the function of spiral vessels, no result has been obtained, nor could any be obtained, because each person meant, quite *ad libitum*, the spiral vessels of herbaceous plants, or of the wood, completely shutting their eyes to the possibility that the two might be exceedingly different things. If, for instance, we consider the cambium in the earliest period in which it begins to acquire organization, we find that it consists throughout of entirely similar prosenchymatous cells still in a gelatinous state. A short time afterwards some longitudinal series of these cells appear to have increased in breadth, by which alone they are distinguishable from the adjacent mass. On a further development we observe that some dark spots appear on the walls of some of these expanded cells, which we soon recognise to be small flat air-bubbles that have formed between the walls of this and of the neighbouring cell. Gradually all the expanded cells which are superposed one upon the other are changed in this way; the air-bubble gradually appears more circularly or ovately bounded, and there appears in its centre a smaller circle which constantly becomes more distinct, and which originates in the following manner:—on the deposition of new masses upon the inner wall of the cell, the parts corresponding to the outer air-bubble remain free from this deposition, thus forming a small canal which traverses the newly deposited mass. We now distinguish the fully developed porous vessel, the septa between each two superposed cells appearing at the same time to be more or less reabsorbed. This history of the formation of the porous vessels, which may easily be observed on limes and willows, greatly contradicts the general notion that the porous canals serve to facilitate the communication of the saps. As the air-bubble is first formed on the outer surface of the wall, it renders the passage of the sap impossible at this spot, and for this reason the origin of the porous canal might probably be most easily and naturally explained as a local atrophy of the cellular wall. At the same time it is evident from hence that the distinction between wood in general [*laubholz*] and fir-wood, as to its anatomical structure, cannot be of such vast physiological importance; for, with like elements and

like development, the distinction depends in fact on the larger or smaller number of cells that are converted into porous vessels.

There are, however, a vast number of gaps still to fill up: and, more especially, the origin of the medullary rays and their relation to the wood, the formation of the new bark, and lastly, the *origin* of the buds in the wood, are so many questions for extensive researches, to the execution of which, however, we may look forward at no distant time, considering the ardent and gratifying zeal which has been awakened and cherished, especially among our contemporaries, in behalf of the sound and scientific study of the anatomy and physiology of plants.

I have, as far as lay in my power, attempted in this Memoir to solve many interesting questions in vegetable physiology; or, by more accurate definitions of the question, to advance nearer to a future solution. May these observations meet with a friendly reception, and be speedily improved upon and extended among the vegetable physiologists of Germany.

EXPLANATION OF PLATES XV. AND XVI.

Fig. 1. Cellular tissue of the albumen from the embryo-sac of *Chamædorea Schiedeana* in the act of formation. *a*. The inner mass consisting of gum with intermixed mucous granules and cytoblasts. *b*. New cells, still soluble in distilled water. *c—e*. Further development of the cells, which by a slight pressure still form into an amorphous gelatinous mass, with the exception of the cytoblasts.

Fig. 2. The formative substance from fig. 1. *a*. more highly magnified, gum, mucous granules, nuclei of the cytoblasts and cytoblasts.

Fig. 3. A single, still free cytoblast, still more highly magnified.

Fig. 4. A cytoblast with the cell forming on it.

Fig. 5. The same, more highly magnified.

Fig. 6. The same. The cytoblast here exhibits two nuclei and is represented in Fig. 7. in an isolated state after the destruction of the cell by pressure.

Fig. 8. The same cellular tissue still further advanced in development than in Fig. 1. *e*. The walls of the cells in contact already cohere. In *a*. their horizontal section, it may be distinctly perceived that the cytoblast is inclosed in the cellular wall.

Fig. 9. Cells of the nearly mature albumen in a thin cross section.

Fig. 10. Common septum between two cells from Fig. 9. under

a higher power. The strata-like depositions (near *b.*) upon the inner wall, and the porous canals (near *a.*) produced by their local failure are apparent. I could distinctly count nine to twelve layers which had originated within fourteen days.

Fig. 11. A spore from *Rhizina lævigata*, Fries, with the cytoblasts.

Figs. 12—14. Several cytoblasts from the embryo sac of *Pimelea drupacea* before the appearance of cells.

Fig. 15. Young cells with their cytoblasts from the same. The latter here unquestionably present three nuclei.

Fig. 16. A portion of the embryonal end of the pollen-tube projecting from the ovulum in *Orchis Morio*, in which towards the upper part cells have already developed. Below, the original pollen-tube is still distinguishable. The almost globular cytoblasts in this case are distinctly included in the cellular wall.

Fig. 17. Embryonal end of the pollen-tube from *Linum pallescens*, together with the appended lobule of the embryo sac (*a.*). The process of the formation of cells is in its beginning. Above, a young cell with its cytoblasts is already perceptible; beneath this are seen several cellular nuclei floating in a free state.

Fig. 18—20. Commencement of the germination in the spores of *Marchantia polymorpha*. Compare the text, p. 297.

Fig. 21. Portions of the pollen-tube become cellular in *Orchis latifolia* in the highest stage of development. The covering derived from the pollen-tube is no longer perceptible. The cytoblast is exactly as in Fig. 16. included in the wall of the cell.

Figs. 22 and 23. Two isolated cells from the terminal shoot (*punctum vegetationis*, Wolff.) of *Gasteria racemosa*; in 22, two free cytoblasts are seen; in 23, two newly formed cells in the original cell.

Fig. 24. A very young leaf of *Crassula portulacea*, the five cells solely composing it are still surrounded by an original cell.

Fig. 25. Three cells from an articulated hair of a potatoe, with a quantity of currents of mucus at the sides, giving them a reticulate appearance. In the middle cell the direction of the currents is partly indicated by arrows.

Wherever hitherto I have observed in Phanerogamia these movements in the cells, I have constantly found that the moving part consisted of a yellowish gelatinous fluid, perfectly insoluble in distilled water, and mixed with a quantity of minute black granules, differing entirely from the other aqueous cellular sap; and even where the currents were so minute that they appeared merely as excessively minute delicate lines of black points, yet I succeeded with higher magnifying powers in distinguishing the yellowish gelatinous fluid, especially with the favourable circumstance, which frequently occurs, of the current being arrested by some preventive, thus causing a somewhat large quan-

tity of the moving water to be aggregated, and upon this followed in general either a change of direction or a division of the current.

Fig. 26. Cells from the epidermis of the pericarp of *Ocymum basilicum*, moistened with water, so that the globule of mucus has expanded, and has torn the outer cellular wall (*a*) from the side walls (*b*).

Fig. 27. Cells from the epidermis of the pericarp of *Ziziphora dasyantha*.

Fig. 28. Cells from the epidermis of the pericarp of *Salvia verticillata*.

Fig. 29. Cells from the epidermis of the pericarp of *Salvia Horminum*.

Fig. 30. Cells from the epidermis of the pericarp of *Salvia Spielmanni*.

Fig. 27—30 *a*. shew the remains of the side walls of the ruptured cells.

Fig. 31. A portion of the epidermis (*a*) and of the integument (*b*) of the ovule of *Collomia coccinea*. The epidermis cells only contain granules of starch.

Fig. 32. The cells of the epidermis of the half-ripe seed of the same plant, containing mostly gum, near *a*. some still undecomposed starch.

Fig. 33. The same cells in the nearly ripe seed. Beautiful spiral fibres have been formed from the entirely consumed contents.

Fig. 34. Cells of the epidermis of the seed of *Leptosiphon androsaceum*, moistened with water, so that the globule of gelatine has come out. (*a*) remains of the cellular walls.

Fig. 35. Cells from the epidermis of the seed of *Hydrocharis morsus ranæ*. In the lower part of the cells, where they are connected with one another, the spiral coils take a direction different from that in the upper free part of the cells.

For Figs. 26—35 compare the text p. 293 to p. 295.

SCIENTIFIC MEMOIRS.

VOL. II.—PART VII.

ARTICLE IX.

Supplement to the Treatise entitled "General Theory of Terrestrial Magnetism†." By Professor C. F. GAUSS, of the University of Göttingen.

[Translated from the *Resultate aus den Beobachtungen des magnetischen Vereins im Jahre* 1838. The references in the translation of the "Supplement" are made to the corresponding pages of the translation of the General Theory in the Scientific Memoirs.]

AFTER the table of comparisons, pp. 216—219, was printed, two slight inaccuracies were remarked in it:—the one at Callao arose from a wrong longitude in the work referred to in page 222; that at St. Helena arose from an error of calculation. I have subjoined the corrected result at those two places, and have availed myself of this opportunity to give the comparison of the theory with observations at eight other stations which have since come to my knowledge.

	Station.	Latitude.	Longitude.	Declination. Computed.	Declination. Observed.	Declination. Difference.
		° ′	° ′	° ′	° ′	° ′
8*	Port Etches	+60 21	213 19	−28 33	−31 38	+3 5
8**	Lerwick............	+60 9	358 53	+27 10	+27 16	−0 6
11*	Stockholm.........	+59 20	18 4	+15 22	+14 57	+0 25
34*	Valencia	+51 56	349 43	+30 2	+28 43	+1 19
40*	Brussels............	+50 52	4 50	+23 23	+22 19	+1 4
54*	Montreal	+45 27	286 30	+ 5 23	+ 7 30	−2 7
62*	Oahu	+21 17	202 0	−12 19	−10 40	−1 39
64*	Panama............	+ 8 37	280 31	− 6 44	− 7 37	+0 53
68	Callao	−12 4	282 52	− 9 32	−10 0	+0 28
71	St. Helena.........	−15 55	354 17	+19 27	+18 0	+1 27

† Translated in the Scientific Memoirs, vol. ii., Art. V.

	Inclination.			Intensity.		
	Computed.	Observed.	Difference.	Computed.	Observed.	Difference.
	° ′	° ′	° ′			
8*	+ 76 25	+ 76 3	+ 0 22	1·678	1·75	− 0·072
8**	+ 73 46	+ 73 45	+ 0 1	1·469	1·421	+ 0·048
11*	+ 70 52	+ 71 40	+ 0 48	1·451	1·382	+ 0·069
34*	+ 71 25	+ 70 52	+ 0 33	1·448	1·409	+ 0·039
40*	+ 67 29	+ 68 49	− 1 20	1·393	1·369	+ 0·024
54*	+ 77 24	+ 76 19	+ 1 5	1·713	1·805	− 0·092
62*	+ 37 36	+ 41 35	− 3 59	1·125	1·14	− 0·015
64*	+ 34 40	+ 31 55	+ 2 45	1·238	1·19	+ 0·048
68	− 4 39	− 6 14	+ 1 35	1·003	0·97	+ 0·033
71	− 14 52	− 18 1	+ 3 9	0·811	0·836	− 0·025

The observations at Stockholm were made by Rudberg; those of intensity and inclination in 1832, and those of declination in 1833: *Poggendorff's Annals*, vol. xxxvii. The observations at Brussels are for 1832; those of declination and inclination are by Quetelet, (*Bulletin de l'Académie de Bruxelles*, tome vi.,) those of intensity by Rudberg, (*Sabine, Report on the Variations of the Magnetic Intensity*). I am indebted to the obliging communication of Major Sabine for the determinations at the other six new stations, as well as for the intensity at Callao, and for a more recent determination of the dip at that place. The observations at Lerwick and Valencia were made by Captain James Ross in 1838; those at Port Etches, Panama and Oahu, by Captain Belcher in 1837; and those at Callao by the same officer in 1838: the inclination and intensity at Montreal were observed by Major Estcourt in 1838; the declination at that station is for 1834, but the observer is not named.

There are two other trifling corrections to be made in the table of comparisons. By an error of the press, the longitude of Naples is made 10′ too small, although the true longitude 14° 16′, was employed in the calculation.

The declination observed by FitzRoy at Otaheite is printed in one part of the *Magnetic Observations made during the Voyages of H. B. M.'s ships Adventure and Beagle* as 7° 34′ E., in another part of the same work 7° 54′ E. Of these two numbers, the one employed in the table of comparisons was the erroneous one; the difference between calculation and observation at that station is therefore + 2° 0·9′.

The following errors of the press are also to be corrected:—

Page 186, line the last, *for* twelve *read* fourteen.

Page 239, in $\phi = 45°$, log. a^{I}, *for* 2·29724 *read* 2·29796.
— 251, in $\phi = -13°$, log. c^{IV}, *for* 1·27047 *read* 1·37047.

The public is indebted to M. Weber for the map (Pl. XVII.) containing the values of the declination as computed from the Elements of the Theory of Terrestrial Magnetism (Scien. Mem. vol. ii. page 211). In order to give a clearer view of the intricate form of the system of lines of equal declination, the points at which the declination has a maximum value, as well as those points where two lines of equal declination intersect each other, (or where one such line crosses itself,) have been computed with especial care. There are two points of the first kind, and four of the second kind. The common character of such points consists in this, that the first differential of the declination in every direction disappears.

It is almost superfluous to remark, that in those regions where the declinations alter very slowly on all sides, as in Southern and South-Eastern Asia, small alterations in the values of the declination may produce very great changes in the form of the system of lines.

The same remark applies to the maps of the Total Intensity (Pl. XVIII. and XIX.) computed by Dr. Goldschmidt from the tables, pages 236—251.

These maps show, in the northern hemisphere, two points of maximum intensity, and one point of intersection of lines of equal intensity; in the southern hemisphere, one point of maximum; and in the middle zone, two points of minimum intensity, and two points of intersection.

Similar maps, grounded on the theory, are in preparation for the inclination, for the horizontal intensity, for the three components of the earth's magnetic force (*i. e.* the values of X, Y, and Z), and for that distribution of the magnetic fluids on the surface of the earth, which may be taken as the representative of the actual distribution in the interior. We hope to publish these maps in the *Resultate* for 1839. G.

Note by the Translator.

The maps of the Declination and of the Total Intensity, computed according to M. Gauss's theory, are given in the present number of the Scientific Memoirs. By the kind permission of MM. Gauss and Weber, the translator is also enabled to give in

the present number the maps of the Inclination and of the Horizontal Intensity (two of each), Pl. XX., XXI., XXII., and XXIII., computed also according to M. Gauss's theory, and not yet published in Germany.

It is requested that the following corrections, which have been kindly pointed out by M. Gauss, may be made in the Translation of the "General Theory" (Sc. Mem., vol. ii. Part vi.)

Page 196, line 35, *for* the space included by *read* the space comprehending.

— 196, *note*, line 1, *for* themselves *read* even if.

— 202, line 21,

for $\sqrt{}(r^2 - 2\, r\, r^0) \cos u \cos u^0 + \sin u \sin u^0 \cos(\lambda - \lambda^0) + r^0 r^0$
read $\sqrt{}(r^2 - 2\, r\, r^0 (\cos u \cos u^0 + \sin u \sin u^0 \cos(\lambda - \lambda^0)) + r^0 r^0)$.

— 204, line 4.

In the second term of the factor of $\sin u^m$ *for*

$$-\frac{(n-m)\,(n-m+1)}{2\,(2\,n-1)} \quad \textit{read} \; -\frac{(n-m)\,(n-m-1)}{2\,(2\,n-1)}.$$

— 210, line 35, *for* on seven parallels *read* on each of seven parallels.

— 228, line 16, *for* the present century *read* future centuries.

— 236, line 38, *for* eliminate *read* obtain.

It is also requested that the following corrections may be made in the magnetic papers in the Sc. Mem. vol. ii. part 5 :—

Page 57, line 17 from bottom, *for* immediately following *read* nearest.

— 80, lines 11 and 12, *for* $B'^2 - B''^2$, *read* $B'^2 - B\,B''$.

— 82, last line but two, in the value of r, *for* $+$ *read* $\times$ throughout.

— 83, line 13, in the value of C, *for* $+$ *read* $\times$ throughout.

Reference to the Plates.

XVII. Map of the Lines of Declination.
XVIII. Map of the Lines of Total Intensity: Part 1.
XIX. Ditto: Parts 2 and 3.
XX. Map of the Lines of Inclination: Part 1.
XXI. Ditto: Parts 2 and 3.
XXII. Map of the Lines of Horizontal Intensity: Part 1.
XXIII. Ditto: Parts 2 and 3.

Article X.

On the Method of Least Squares. *By* J. F. Encke, Director of the Astronomical Observatory at Berlin.

[From the *Astronomisches Jahrbuch* for 1834.]

THE frequent application of the method of least squares, or of the calculus of probabilities, to the results of observations, induces me to hope that a useful service may be rendered, by giving as brief and elementary a view as is possible of the propositions on which this method is founded,—adding thereto certain rules which I know from much experience to be most convenient in practical application. With this design I have drawn the present paper from the following sources: Gauss, *Theoria motus corporum cœlestium,* lib. ii. sect. 3; *Disquisitio de elementis ellipticis Palladis. Com. Gött. recen.* vol. i. 1808–1811; Lindenau *and* Bohnenberger, *Zeitschrift für Astronomie und verwandte Wissenschaften,* bd. i. pp. 185, *et seq.; Theoria combinationis observationum erroribus minimis obnoxiæ. Com. Gött. recen.* 1821 and 1823, Parts I. and II.; combined with remarks by Bessel in the *Fundamenta Astronomiæ,* pp. 18 and 116, and in his treatise on the *Comet of Olbers.* No proposition of any importance is here put forward which is not taken from the above-mentioned sources; but the form of the demonstration has occasionally been altered with the view of rendering it more easy of comprehension. I have not thought it necessary to refer to the particular places where the several propositions are to be found.

The classical labours of other mathematicians, especially those of Laplace and Poisson, agree perfectly with those here given, as respects the results: the form of representation and the mode of deduction are different, chiefly because Laplace confined himself to a strictly theoretical view of the subject, and appears to have viewed but one amongst the many applications of the calculus of probabilities. For the present object, it has been thought preferable to follow the path pursued by the two above-named astronomers, who combine the strictest theory with the happiest practical application of theoretical truths;

a rare combination, but of high importance in the cultivation of modern astronomy.

Experience shows that in the simplest kind of observations, and with the utmost care to avoid all circumstances which may occasion error, continued repetitions of the same observations always give results differing somewhat from each other. The causes of these differences are unknown to us; or, if we choose to ascribe them to the imperfection of our instruments, and to the uncertainty of all the perceptions of sense, at least their action cannot be subjected to calculation. We may however assume, that in a certain kind of observation, both the number of the sources of error, and the number of combinations of which they are susceptible, remain the same; and also that the same combination, whenever it occurs, will produce the same error. If we knew the number of all the possible combinations of the sources of error, and if we knew how often those combinations which produce equal errors are contained in this number, we should be enabled, by the calculus of probabilities, to compute *à priori* how often a certain error ought to appear in a given number of observations, and we could calculate the probability that it would not appear more or less frequently than a certain number of times. The causes being unknown, we may, on the other hand, apply the calculation of probabilities to the results of experiments; or, from the number of times that an error has actually appeared in a number of observations, we may infer how often it should have appeared according to rule, and how often it would appear in future repetitions. This application only supposes that the continued repetition does not bring in any new source of error. The number of the sources of error, and of their combinations, remains wholly undetermined.

By the probability of a certain combination, or of all the combinations which produce an error of a certain amount, we understand the proportion which the number of such combinations bears to the number expressing all possible combinations. On this proportion the probability of an error Δ will depend. If this probability (which is necessarily a function of Δ, and of one or more constants having reference to the

kind of observation) be designated generally by $\phi\ \Delta$, then among m observed errors there will be, according to probability, $m\ \phi\ \Delta$ errors of the value Δ; and this determination will be so much nearer the truth as m is greater; so that if m be indefinitely increased, there can be no assignable difference between the value of $m\ \phi\ \Delta$, and the true number of the errors Δ.

Even with this indeterminate designation some of the properties of the function $\phi\ \Delta$ can be shown. We know that in each kind of observation the errors can in no case go beyond a certain, though not precisely definable limit; consequently, if a denote the value of this limit, for $\Delta > a$ (abstracting signs) $\phi\ \Delta$ becomes impossible, or $= 0$. In like manner, on the supposition of the greatest possible care in the observations, and with the assumption, which is the only warrant of certainty in experimental science, that a greater number of observations gives hope of a more exact result,—it is implied that $\phi\ \Delta$ is a maximum for $\Delta = 0$, and is equal for equal positive and negative values. If indeed this were not the case in a continued repetition of the observations, the erroneous values of the quantity to be determined would prevail so much on either the positive or the negative side, that we should find ourselves in the impossibility of attaining the truth, and should be in danger, even with an infinite number of observations, of taking an erroneous value for the most probable one. We have then as the most probable value resulting from our observations, that for which $\phi\ \Delta$ is a maximum with $\Delta = 0$, and which is besides a direct function of Δ; and as we have no other means than the observations of determining the true value, this value must be to us the true one.

In these assumptions, however, the distinction between constant and irregular errors requires consideration. By constant errors, are generally understood those of which the sources are not general, but belong to the particular observations, sometimes to a particular instrument, or to the individuality of the observer. Irregular errors, on the other hand, are those which occur under all circumstances, and which are therefore properly subject to the calculus of probabilities. The causes of the *smaller* constant errors are in themselves analogous to those which produce the irregular errors, and the total avoidance of them may even be regarded as impossible. Our aim should be

to avoid wholly the greater constant errors,—or to lessen them as much as possible,—or to bring their influence so far within the power of computation, that the remaining constant errors in one mode of making the observations may appertain to those sources of error which in other modes of observing can exist only in a different degree. In this case, it is as important to multiply the methods themselves as the observations in each; and by making as many repetitions as possible, and by varying the methods themselves as much as possible, the nearest approach is made to the truth. This distinction between constant and irregular errors does not influence the application of the calculus of probabilities, so long as we do not know whether any and what constant errors exist. Their existence may be ascertained, if, on comparing together the results of different methods, we find that a greater difference exists between them, than the treatment of the observations by each method separately would justify us in expecting. For the most part, the multiplication of the observations according to one method is easier to obtain, and is more frequently met with, than the multiplication of the methods themselves. On this account the result deduced as most probable is usually a partial one; and, in order to come as near as possible to the pure truth, the chief object of attention should be to avoid every possible constant error. In the sequel this distinction will be disregarded; it only causes the estimation of the exactness of such a partial result to be always somewhat faulty,—a circumstance so much the less influential on the general consideration, as the estimation itself lays no claim to absolute certainty.

If now, with the following conditions, $\phi\,\Delta$ a maximum for $\Delta = 0$, $\phi\,\Delta$ an even function of Δ, and $\phi\,\Delta = 0$ for $\Delta > a$,—we combine the remark drawn from experience, that in general smaller errors are more frequent than greater ones,—that in approaching a, the extreme limit, the number of errors decreases with great rapidity,—and that between $\Delta = 0$ and $\Delta = a$ there is in general no value of Δ for which $\phi\,\Delta$ is impossible, or that all errors from 0 to a may exist,—then the march of the function may be assigned *à priori*. A geometric consideration may be here employed to facilitate the conception of it. If the values of Δ be taken as abscissæ, and the $\phi\,\Delta$ belonging to them as rectangular ordinates, the curve of probabilities on both sides of the axis of ordinates will be

symmetrical. A *maximum maximorum* will be found at $\Delta = 0$. From this point forward, according to the law, the curve will be drawn continuously, so that in the neighbourhood of $\Delta = a$, it will approach the axis of abscissæ very rapidly. Hence follows another circumstance of great importance in the sequel. The absolute limit a can never be strictly determined: but as in the neighbourhood of a the ordinates $\phi\,\Delta$ decrease very rapidly, we may without any sensible error assume the limits $-\infty$ and $+\infty$, instead of the values of a, provided the function, which within the values 0 and a should agree with the march of the curve, has the property of decreasing constantly as Δ increases. For in the rapid approach to the axis of the abscissæ, so soon as Δ approaches a, each function which beyond a decreases still more, and was before approaching rapidly, will give for its values between $\pm a$ and $\pm \infty$ only insensible magnitudes.

The definition of $\phi\,\Delta$ implies, that when the number of observations is so great that all errors will occur, each in due proportion of frequency,

$$m\,\phi\,\Delta + m\,\phi\,\Delta' + m\,\phi\,\Delta'' \;.\;\;.\;\;.\;\;.\; = m,$$

or

$$\sum_{-\infty}^{+\infty} (\phi\,\Delta) = 1.$$

Hence we perceive that if the number of Δ be infinite, when all the gradations from $\Delta = 0$ to $\Delta = a$ are taken into account, the function $\phi\,\Delta$ will be infinitely small for any given error Δ. We may express this condition more conveniently, in the language of analysis, by not considering the probability of one determinate error only, but the probability of all the errors lying between the infinitely near limits Δ and $\Delta + d\,\Delta$. Within these infinitely near limits, the value of $\phi\,\Delta$ may be regarded as constant. Hence the probability of the errors between Δ and $\Delta + d\,\Delta$ is $\phi\,\Delta\,d\,\Delta$; and the probability of the errors between the limits a and b is equal to the sum of these elements within the given limits, or

$$= \int_a^b \phi\,\Delta\,d\,\Delta. \qquad (1.)$$

For the limits $-\infty$ and $+\infty$, which include all errors, it becomes

$$\int_{-\infty}^{+\infty} \phi\,\Delta\,d\,\Delta = 1, \qquad (2.)$$

equal to certainty.

The last integral gives the area of the curve of probability taken from the axis of the abscissæ to the curve. It represents the number of observations which are possible, and embrace all errors. Each element of surface $\phi \Delta d \Delta$ compared with the whole surface, shows the proportion which the number of observations giving errors between Δ and $\Delta + d\Delta$ bears to the total number of observations; or it gives the probable number of observations charged with these errors, the whole number being $= 1$.

The object of every observation is the deduction of one or more quantities, by which the observed phenomenon is produced. In the places of the planets, for example, these magnitudes may be the elements of the paths of the planets and of the earth. The manner of combining the elements so as to obtain the observed value must be supposed known, if we wish to determine the value of the elements from observation; therefore every observed quantity M will give an equation

$$M = f(x, y, z, \ldots)$$

where the function f is known, and x, y, z are to be determined according to their most probable values. The equality will be more or less presented according to the values assumed for x, y, z. If we suppose $x = p$, $y = q$, $z = r$, and if

$$V = f(p, q, r),$$

then $M - V$ would be the error of the observations in case the values p, q, r were the true ones.

If several observations of the same kind have been made, in which all the same elements p, q, r determine the observed value, then, in similar manner, by the assumption of $x = p$, $y = q$, $z = r$, the errors $M' - V'$, $M'' - V''$, $M''' - V'''$ will be obtained. By another assumption, $x = p'$, $y = q'$, $z = r'$, substituted in the same manner in all the equations, other values of V, and consequently also other values of $M - V$ will be obtained, so that to every hypothesis as to the value of x, y, z, appertains a determinate system of errors Δ, Δ', Δ'', which depend on the hypothesis. In order to determine from hence the most probable values of x, y, z, we need two propositions from the calculus of probabilities, one of which gives the probability of a connected system of errors when the probability of each single one is known; the other teaches how to determine the probability of the hypothesis from the probability of the system of errors belonging to it.

For the first proposition the calculus of probabilities gives the following expression.

I.

If $\phi\,\Delta$ is the probability of the error Δ, $\phi\,\Delta'$ that of Δ', and so on, then the probability of the concurrence of the errors Δ, Δ', Δ'', &c. is

$$= \phi\,\Delta \,.\, \phi\,\Delta' \,.\, \phi\,\Delta'' \ldots$$

We may convince ourselves of the truth of this in the following manner. Let us assume, for instance, that in three observations the error Δ be found twice, and the error Δ' once; further, let $\phi\,\Delta = \frac{p}{n}$, $\phi\,\Delta' = \frac{q}{n}$. Let the three observations be regarded as belonging to a series of observations, m, so extensive that in it all errors shall occur according to their probability; consequently, $\frac{p}{n}\,m$ errors equal to Δ, $\frac{q}{n}\,m$ errors equal to Δ', will occur in it. Let the number of the remainder be s, in which it is here indifferent how many equal or unequal there are among them. Apart from s, the number of all possible arrangements of the errors in the m observations will be

$$\frac{1\,.\,2\,.\,3\ldots m}{1\,.\,2\,.\,3\ldots\frac{p}{n}\,m\,.\,1.2.3\ldots\,\frac{q}{n}\,m.}$$

As three places are taken up by the two Δ and one Δ', there are left for the remaining $m-3$ observations,

$$\frac{1\,.\,2\,.\,3\ldots\ldots(m-3)}{1\,.\,2\,.\,3\,..\left(\frac{p}{n}\,m-2\right).1\,.\,2\,.\,3\,.\,0..\left(\frac{q}{n}\,m-1\right)}$$

possible mutations. Consequently the probability that in any three observations two Δ and one Δ' should be found

$$=\frac{\left(\frac{p}{n}\,m-1\right)\,.\,\frac{p}{n}\,m\,.\,\frac{q}{n}\,m}{(m-2)\,.\,(m-1)\,.\,m},$$

or

$$=\frac{\left(\frac{p}{n}-\frac{1}{m}\right)\,.\,\frac{p}{n}\,.\,\frac{q}{n}}{\left(1-\frac{2}{m}\right)\,.\,\left(1-\frac{1}{m}\right)\,.\,1}$$

The assumption on which we have proceeded is, however, strictly true only for $m=\infty$, or the probability of a single com-

bination of two Δ and one Δ' in any otherwise arbitrary arrangement

$$= (\phi\,\Delta)^2 \phi\,\Delta',$$

whence the above formula is deducible.

To obtain the second proposition, let us consider the case in which any observation has given the value of M. Now compare together two hypotheses as to x, y, z. Let

$$\begin{aligned} \text{Hyp. I.} \ldots\ldots & x = p,\ y = q,\ z = r \\ \text{II.} \ldots\ldots & x = p',\ y = q',\ z = r'. \end{aligned}$$

Before M is observed, we have no measure of the relative probabilities of these two hypotheses, or of any others; therefore, before the observation they must be regarded as equally probable. But after M has been found, Hyp. I. will give the error Δ, with the probability $\phi\,\Delta$, and Hyp. II. will give the error Δ', with the probability $\phi\,\Delta'$. If we denote by m the number of cases in which, assuming Hyp. I., M will proceed from it, and by n the number of cases in which, by the same supposition, M will not be obtained, then will

$$\phi\,\Delta = \frac{m}{m+n}.$$

Let m' and n' have the same signification in Hyp. II., then

$$\phi\,\Delta' = \frac{m'}{m'+n'}.$$

But besides these two suppositions, of either Hyp. I. or Hyp. II. being the true one, there are also cases in which neither are true, and amongst these there may be some which, in certain cases, give M. Let the signification of m'' and n'' for all other hypotheses be the same as above, then the number of all possible cases will be $= m + n + m' + n' + m'' + n''$; therefore the probability of Hyp. I., before the observation is made,

$$= \frac{m+n}{m+n+m'+n'+m''+n''},$$

and that of Hyp. II., before the observation is made,

$$= \frac{m'+n'}{m+n+m'+n'+m''+n''};$$

these two values must be considered equal, whence it follows that

$$m + n = m' + n'.$$

But after M has actually been found, the cases where it does

not result are excluded; consequently, in reference to the observed value M, the relative probability of Hyp. I.

$$= \frac{m}{m + m' + m''},$$

and that of Hyp. II.

$$= \frac{m'}{m + m' + m''},$$

or they are to each other as $m : m'$, and in consequence of the equation $m + n = m' + n'$, as $\frac{m}{m + n} : \frac{m'}{m' + n'}$, or as $\phi\,\Delta : \phi\,\Delta'$. Hence follows the proposition:

II.

The probabilities of two hypotheses, which are equally probable before the observation is made, and which exclude each other, are directly proportional to the probability of the errors, or system of errors, proceeding from them.

Consequently, if the magnitudes M are found by a kind of observation of which it is by other means known what errors may occur in it, and in what proportion, or for which the law of the probability of the errors $\phi\,\Delta$ is known, (which is independent of the use to be afterwards made of these observations for determining one or more unknown values,) then the probability of each hypothesis as to x, y, z, is proportional to the product

$$\phi\,\Delta \,.\, \phi\,\Delta' \,.\, \phi\,\Delta'' \,.\, \phi\,\Delta''' \,\ldots\ldots = \Omega, \qquad (3.)$$

where $\Delta, \Delta', \Delta'', \Delta'''$ are the errors which remain over in each hypothesis. The most probable hypothesis will be that in which Ω is a maximum, or in which, in differentiating, $d\,\Omega$ becomes $= 0$. On account of the mutual independence of the quantities x, y, z, this equation divides itself into the separate equations $\frac{d\,\Omega}{d\,x} = 0$, $\frac{d\,\Omega}{d\,y} = 0$, $\frac{d\,\Omega}{d\,z} = 0$.

Generally, each

$$\Delta = \mathrm{M} - \mathrm{V}.$$

If consequently, before the substitution of a numerical value for x, y, z, the functions $\mathrm{M} - \mathrm{V}$ be designated by v, so that

$$\mathrm{M} - \mathrm{V} = v,\ \mathrm{M}' - \mathrm{V}' = v',\ \mathrm{M}'' - \mathrm{V}'' = v'', \text{ \&c.};$$

and if, for the sake of easier differentiation, we make

$$\log.\,\Omega = \log.\,\phi\,\Delta + \log.\,\phi\,\Delta' + \log.\,\phi\,\Delta''' \ldots\ldots$$

and designate the logarithmic differential by $\phi' \Delta$, so that

$$\frac{d \phi \Delta}{\phi \Delta \, d \Delta} = \phi' \Delta,$$

the equations of condition of the maximum become the following:

$$\frac{d v}{d x} \phi' v + \frac{d v'}{d x} \phi' v' + \frac{d v''}{d x} \phi' v'' + \frac{d v'''}{d x} \phi' v''' \ldots . = 0,$$

$$\frac{d v}{d y} \phi' v + \frac{d v'}{d y} \phi' v' + \frac{d v''}{d y} \phi' v'' + \frac{d v'''}{d y} \phi' v''' \ldots . = 0,$$

$$\frac{d v}{d z} \phi' v + \frac{d v'}{d z} \phi' v' + \frac{d v''}{d z} \phi' v'' + \frac{d v'''}{d z} \phi' v''' \ldots . = 0;$$

whence the values of x, y, z, which satisfy them, and which consequently are the most probable values, must be determined.

These general propositions can, however, only be applied when the function ϕ is known in each separate case. Instead of making different hypotheses as to its most appropriate form, and then trying which of these corresponds best with experience, we shall attain our object more directly, by considering in a converse manner the simplest case,—examining for it what values experience (apart from the general formulæ of the calculus of probabilities) teaches us to prefer,—and then trying to determine from thence the form of ϕ by means of the general formulæ.

Let us suppose any arbitrary number of observations, all made under equal circumstances, so that beforehand no preference can be given to any one above the rest. Let us say that these observations are to be applied to the determination of the value of an unknown quantity, of which the true value would be given directly by each single observation, if there were no errors of observation. An examination of the difference between two right lines may serve as an example.

First, if *one* observation has been made, giving the value a, there is no choice but to put

$$x = a.$$

If *two* observations have given the values a and b, and if neither of these is to be preferred to the other, then from these observations *alone* the value of x must be determined in such manner that the differences $x - a$ and $x - b$ may come out equal. This gives

$$x = \tfrac{1}{2} (a + b),$$

under the supposition that a positive and a negative deviation,

of equal absolute amount, are to be viewed as equal errors. If a fundamental principle is necessarily required, this supposition appears to be the simplest of all. It rests on the consciousness of having exercised the greatest possible care, so that no reason exists for assuming that an error has been made, either in the positive or in the negative sense. But let it even be granted that an error tends to occur more frequently in one sense than in the other, still, so long as we do not know in which sense it occurs, the value $\frac{1}{2}(a + b)$ is the only one which in this uncertainty will make the error of the result the smallest; or, at least, which will most securely avoid the danger of increasing the error.

Now let *three* observations have been made. On account of the fully equal worth of the observations, the values found, a, b, c, must be so combined that no one shall influence the result more than another, independently of their numerical values. Or it must be assumed that

$$x = \text{symmetric function } (a, b, c).$$

But we may consider the subject in another point of view. If two of the observations alone be taken, we should have, according to which two were selected for that purpose, one of the three following results, which in each case would be the only result that could be chosen:—

$$\tfrac{1}{2}(a + b), \quad \tfrac{1}{2}(a + c), \quad \tfrac{1}{2}(b + c).$$

To this the third observation adds c, b, a. It is true, that we can no longer combine the two values in each arrangement symmetrically, because one rests on two observations, the other on one. But whatever may be the form for the combination of both, it must unquestionably be that, which would produce the result to which the preference is due as derived from the *three* observations; and this form, which may be arbitrarily designated by ψ, must be the same for all three. Hence we have for x the three values

$$\begin{aligned} x &= \psi\left(\tfrac{1}{2}(a + b),\ c\right), \\ &= \psi\left(\tfrac{1}{2}(a + c),\ b\right), \\ &= \psi\left(\tfrac{1}{2}(b + c),\ a\right). \end{aligned}$$

If we introduce here the sum of a, b, c, or if we say

$$a + b + c = s,$$

then

$$x = \psi\ (\tfrac{1}{2}\ (s - c), c) = \psi\ (s, c),$$
$$= \psi\ (\tfrac{1}{2}\ (s - b), b) = \psi\ (s, b),$$
$$= \psi\ (\tfrac{1}{2}\ (s - a), a) = \psi\ (s, a).$$

But these three formulæ, from what has been said above, should give a symmetrical form to x in reference to a, b, c, which, as s is already in itself a symmetrical form, can only be if c, b, a disappear in the development by the powers of s; consequently, in the same manner, from all three,

$$x = \psi\ (s).$$

If now in a given case $a = b = c$, the only possible value of x would be $x = a$; consequently,

$$a = \psi\ (3\,a),$$

or the function sign ψ would signify the dividing by 3. Hence follows,

$$x = \frac{a + b + c}{3}$$

for three observations.

In like manner it follows generally, that if for n observations, the value to be chosen is

$$x = \frac{a + b + c \ldots . + n}{n};$$

then, if another observation p is added, for $(n + 1)$ observations,

$$x = \frac{a + b + c \ldots . + n + p}{n + 1}$$

ought to be chosen; for the equality of the observations requires that if we put

$$a + b + c \ldots . + n + p = s,$$

then

$$x = \psi\ \left(\frac{1}{n}(s-p), p\right), \text{\&c.}$$

should be a symmetrical function of all the $n+1$ values. Now, as this form is good for three values, it follows that it is so also for any number of observations, great or small.

This proposition,—that in any number of equally good observations of an unknown quantity, the arithmetical mean of all gives the value which is to be preferred, and which consequently must be regarded as the most probable value,—has been received as a fundamental proposition ever since combinations of

several observations have been made. Rightly understood, the confidence which we place in all quantities derived by experiment in any science, rests essentially on this proposition; it may, therefore, safely be affirmed concerning it, that its truth has been confirmed by experience. The deduction here given shows somewhat more clearly than would be done by the simple statement of the proposition itself, the suppositions on which it is founded. If the observations are made under strictly equal circumstances, and if in two observations a positive and a negative deviation of equal amount are regarded as equal, the arithmetical mean is the only value which does not contradict these suppositions. Then it cannot well be denied that the same value ought to be obtained, whether the observations are considered all together, or divided into arbitrary groups, provided only that no arbitrary supposition is made in the combination of the results of these groups amongst themselves. To deny this, would be to deny that there is any value which ought to be chosen in preference to others. It may perhaps serve to illustrate the importance of the supposition of the equality of the observations in reference to the arithmetical mean, if we refer to the example furnished by Lambert, in the Photométrie, §. 276, in which the arithmetical mean obviously does not give the greatest approximation to the truth. The periphery of a circle is always between the values of the perimeter of an inscribed and a circumscribed polygon of an equal number of sides. If, therefore, we considered the perimeter of a polygon of n sides as an observation of the length of the periphery, and regarded the arithmetical mean between the inscribed and circumscribed polygon of n sides as the most probable value of the periphery, we should be in error. We come nearer the truth if we add to the perimeter of the inscribed polygon the third part of the difference between the two.

Whether, therefore, we regard the principle of the arithmetical mean, in observations of equal worth, as a fundamental proposition which experience has confirmed,—or whether we prefer to take those propositions, on which the deduction here given is based, as more simple fundamental propositions requiring no proof,—in either case the founding of the application of the calculus of probabilities to observations *on the principle of the arithmetical mean* is, perhaps, of all the modes of demonstration, that which is most useful to the practical mathemati-

cian. Therefore we give the following deduction, which is based on proposition

III.

Any number of equally good direct observations of an unknown magnitude being given, the arithmetical mean of all the observed values determines the most probable value of the unknown magnitude, so far as it is determinable by these observations, without requiring or universally admitting any other condition.

Let there be m equally good observations of the unknown magnitude x, and let them have given for it the values M, M′, M″, &c. According to the last proposition, if

$$p = \frac{M + M' + M'' \dots .}{m}$$

the most probable value of x in every case, so far as it can be concluded from these m observations, will be the magnitude p. Consequently $M - p$, $M' - p$, $M'' - p$ must be regarded as errors of observation; or the equation from which the most probable value of x proceeds according to the arithmetical mean, is

$$M - x + M' - x + M'' - x + M''' - x \dots = 0. \quad (4.)$$

If we apply to the same case the general formulæ of the calculus of probabilities, we have

$$v = M - x, \; v' = M' - x, \; v'' = M'' - x \dots .;$$

consequently the only equation of condition of the most probable value is

$$\phi'(M - x) + \phi'(M' - x) + \phi'(M'' - x) + \phi'(M''' - x) \dots = 0,$$

to which the following form may also be given:

$$(M - x,)\frac{\phi'(M - x)}{M - x} + (M' - x)\frac{\phi'(M' - x)}{M' - x}$$
$$+ (M'' - x)\frac{\phi'(M'' - x)}{M'' - x} \dots = 0.$$

It follows immediately from this latter form, that the above equation deduced from the arithmetical mean, will *universally* satisfy this last equation only when

$$\frac{\phi'(M - x)}{M - x} = \frac{\phi'(M' - x)}{M' - x} = \frac{\phi'(M'' - x)}{M'' - x}, \text{ \&c.};$$

i. e. when $\frac{\phi' \Delta}{\Delta}$ is independent of the value of Δ, or when $\frac{\phi' \Delta}{\Delta}$

is equal to a constant. Any function $\frac{\phi'(M-x)}{M-x}$, can only contain, besides this common constant, such members as vary with the value of $(M-x)$, or are a function of $(M-x)$. But whatever function may be assumed, a sum of products of the form $(M-x)\,f\,(M-x)$ will never in general equal $=0$, by virtue of the single equation (4.). For let it be granted that it might happen that for the values $M, M', M'' \ldots$ this sum might, with the equation (4.), $=0$, still in all cases in which, with the unchanged sum $M + M' + M'' \ldots = m\,p$, somewhat different values $M-\alpha$, $M'+\alpha$, $M''-\beta$, $M'''+\beta$, &c. have been found, a new equation would arise, which, if the arithmetical mean holds good, must be, together with the equation (4.), $=0$. But from the infinite diversity which not only *may*, but, according to experience, *will* be found to exist in the amount of the changes of M, M', M'', as well as in their distribution, there can be no function which shall fulfill all these conditions at once. Although the values of $M-p$, $M'-p$, $M''-p$ are not absolutely independent of each other, because p depends on their sum, yet they must, in case the arithmetical mean holds universally good, be considered as independent variables, because the *only* equation which expresses this dependence, with *every* number of observations, disappears in consequence of the infinite diversity of the values which may still be found after this equation has been fulfilled.

This equation,

$$\frac{\phi'\,\Delta}{\Delta} = \frac{d \log.\,(\phi\,\Delta)}{\Delta\, d\,\Delta} = k,$$

where k is an arbitrary constant, gives immediately the form of $\phi\,\Delta$. Integrating,

$$\text{Const.} + \log.\,\phi\,\Delta = \tfrac{1}{2}\,k\,\Delta^2;$$

$$\text{or, } \phi\,\Delta = \varkappa\, e^{\frac{1}{2}k\,\Delta^2};$$

in which formula the value of the constants remains still be to be determined.

In regard to k, the above remark, that $\phi\,\Delta$ must be a maximum for $\Delta = 0$, shows at once that k must always be negative. It may therefore be more convenient to write

$$\phi\,\Delta = \varkappa\, e^{-h^2\,\Delta^2}.$$

The equation (2.)

$$\int_{-\infty}^{+\infty} \phi \, \Delta \, d \, \Delta = 1$$

may then serve for the further determination of a constant. If we make $h \, \Delta = t$, this integral becomes

$$(5) \ldots \quad \frac{\kappa}{h} \int_{-\infty}^{+\infty} e^{-t^2} \, d \, t = 1,$$

where the limits remain the same as before.

In order to obtain the value of this definite integral, let us examine the double integral*

$$V = \int \int_{-\infty}^{+\infty} e^{-(x^2 + y^2)} d \, x \, d \, y,$$

where x and y signify two variable magnitudes independent of each other, and the limits $-\infty$ to $+\infty$ refer to the integration according to x as well as to that according to y. If we integrate first according to y, considering x as constant, and make the value

$$\int_{-\infty}^{+\infty} e^{-y^2} \, d \, y = L,$$

then

$$V = L \int_{-\infty}^{+\infty} e^{-x^2} d \, x$$

consequently, if we now integrate according to x,

$$V = L^2.$$

But we may also compare the expression for V with the general formula for the cubature of a solid. If we consider x, y, z as three rectangular co-ordinates, and imagine the surface the equation of which

$$z = e^{-(x^2 + y^2)},$$

V will express the volume of the body bounded by this infinitely extended surface. But this surface would obviously have arisen by rotation round the axis of the z, because z comes alike to all points of the plane of the x, y, which are equally distant from the point of beginning. On this account the volume of the

* According to the verbal communication to which I am indebted for this short and elegant mode of finding the value of the definite integral, M. Cauchy has given it thus in his lectures.

body may also be expressed by a simple integral, if we imagine it decomposed into a series of infinitely thin cylindrical shells, all perpendicular to the plane of the x, y. If we make

$$r^2 = x^2 + y^2,$$

the solid contents of every such cylindrical shell of infinitely small thickness will be found

$$= 2r\, z\, \pi \,.\, dr,$$

consequently, the volume of the body (for which, in relation to r, we must take the limits 0 to ∞); or

$$\mathrm{V} = \int_0^\infty 2r\pi e^{-r^2}\, dr,$$

for which the integral is immediately found,

$$\mathrm{V} = \pi \int_0^\infty d\,(-e^{-r^2}),$$

or for the given limits,

$$\mathrm{V} = \pi.$$

Hence, by virtue of the above,

$$\mathrm{L} = \sqrt{\pi};$$

and consequently, by substituting this value in (5.),

$$\frac{\varkappa}{h}\sqrt{\pi} = 1,$$

or

$$\varkappa = \frac{h}{\sqrt{\pi}}.$$

The complete expression for $\phi\,\Delta$ will be accordingly

$$\phi\,\Delta = \frac{h}{\sqrt{\pi}}\, e^{-h^2\Delta^2}, \qquad (6.)$$

which not only contains in itself the principle of the arithmetical mean, but depends so immediately upon it, that for all those magnitudes for which the arithmetical mean holds good in the simple cases in which it is principally applied, no other law of probability can be assumed than that which is expressed by this formula. It is therefore not limited to any special kind of observation, but is altogether general. Equally general is the result in regard to Ω, which follows immediately from this form: namely that, *for any arbitrary number of magnitudes to be determined, the most probable values are those which make the sum* $h^2\,\Delta^2 + h'^2\,\Delta'^2 + h''^2\,\Delta''^2 \ldots\ldots$ *a minimum.*

It follows from this formula, that the probability that an error lies between Δ and $\Delta + d\,\Delta$, is

$$= \frac{h}{\sqrt{\pi}} e^{-h^2 \Delta^2} d\,\Delta, \qquad (7.)$$

and the probability that it lies between the arbitrary limits a and b,

$$= \frac{h}{\sqrt{\pi}} \int_{\Delta = a}^{\Delta = b} e^{-h^2 \Delta^2} d\,\Delta. \qquad (8.)$$

Calling the number of the errors unity, this integral expresses also the number of errors which should occur between a and b according to the law, and which will occur very approximately if the number is sufficiently great. If we make

$$h\,\Delta = t,$$

the integral takes the form

$$\frac{1}{\sqrt{\pi}} \int_{t = a h}^{t = b h} e^{-t^2} d\,t.$$

If we take for the limits an equal positive and negative value $-a\,h$ to $+a\,h$, then on account of the even power of t in the differential, we may write

$$\frac{2}{\sqrt{\pi}} \int_{t = 0}^{t = a h} e^{-t^2} d\,t;$$

and we may thence, by means of a table giving this integral for successive values of $a\,h$, obtain a clear representation of the distribution of the errors, without regard to signs, but simply in respect of their magnitudes, proceeding from 0 to the extreme limits. Such a table is appended (Tab. I.). It is deduced directly from the table for the integral $\int e^{-t^2} d\,t$ in Bessel's *Fundamenta Astronomiæ*. The calculation of such a table from the developement of the integral according to ascending and descending powers of t, or according to a continued fraction, is found frequently given, as this remarkable function is applied in many ways in different researches.

This table shows at the same time, how rapidly the number of errors included in equal intervals of the value of t decreases in the higher values. It justifies, therefore, our assumption of the limits $-\infty$ and $+\infty$ in lieu of the actual limits, which must be narrower, although they are not susceptible of being

defined with precision. In a thousand observations there are between

$t = 0$	and	$t = 0{\cdot}5$	520 errors
$t = 0{\cdot}5$	„	$t = 1{\cdot}0$	323 „
$t = 1{\cdot}0$	„	$t = 1{\cdot}5$	123 „
$t = 1{\cdot}5$	„	$t = 2{\cdot}0$	29 „

and between this latter limit and $t = \infty$ there remain only five errors; a number so small that it is scarcely probable that anything will ever be experimentally decided in respect to this deviation of the theory from the rule.

Among the different values of t, there is one especially which may lead to a determinate view as to the proportionate exactness of different kinds of observations. It is that value of t for which the integral has the value 0·5, or which, if we take a sufficiently large number of errors, and imagine them arranged in the order of their magnitudes without respect to signs, will divide them into two parts, each containing an equal number of errors. The number of errors is assumed to be large only in order that the law of probability may actually be fulfilled with sufficient approximation. From the table, it is found, by interpolation, that the value of the integral 0·5 corresponds to the value of $t = 0{\cdot}476936$. Let this number, which holds good for all kinds of observations, be designated by ϱ, on account of the frequent use to be made of it, so that

$$\varrho = 0{\cdot}476936 \text{ and } \frac{2}{\sqrt{\pi}} \int_{t=0}^{t=\varrho} e^{-t^2}\, dt = \tfrac{1}{2}. \qquad (9.)$$

If we designate by r the error which belongs to the value $t = \varrho$ in each kind of observations, then

$$\varrho = h r \text{ or } h = \frac{\varrho}{r}. \qquad (10.)$$

German astronomers call the magnitude r the *probable error* of any particular class of observations*. It is that error *below* which there are as many errors less than itself as there are larger ones *above* it; so that there are as many cases in which the errors are less than r, as there are cases in which the errors are greater. Therefore it is an equal chance, that the error of an

* The French geometricians are in the habit of giving to this value of r the name of *l'erreur moyenne*: this is the more to be borne in mind, as the import hitherto given by German writers to the term *mean error* differs essentially from r.

isolated observation does not exceed r, supposing the value of r for the class of observation to be known.

On account of its frequent use, the value of the integral $\frac{2}{\sqrt{\pi}}\int e^{-t^2}\,d\,t$ has been also given in a second table, arranged according to an argument in which the value of r has been assumed as unity. This table gives for the argument $\frac{\Delta}{r}$ the value of

$$\frac{2}{\sqrt{\pi}}\int_{t=0}^{t=\frac{\varrho\Delta}{r}} e^{-t^2}\,d\,t,$$

so that it shows immediately how many errors will occur up to a determinate error Δ (always without reference to the sign), provided the proportion of the given Δ to the *probable error* be known. It further facilitates a view of the distribution of the errors according to their magnitude. If half the number of all the errors are less than an error $= r$, then among 1000 observed errors, there will be 823 less than $2\,r$, 957 less than $3\,r$, and 993 less than $4\,r$. There will not be more than one error greater than $5\,r$.

By means of this view of the probable error, we may also obtain a clearer view of the signification of the constant h. In different kinds or sets of observations the errors always follow the same law, which is expressed by $\phi\,\Delta$. The difference of any one kind or set from all others depends, therefore, solely on the value of the constants h, and these again afford the means of comparing together observations of different kinds in respect to exactness, and thus enabling them to be subsequently combined.

With two kinds of observations, one of which has the constant h and the other the constant h', the integral $\int\phi\,\Delta\,d\,\Delta$, taken up to any assigned limit, will have equal values, if the value of the limit, determined in both cases by the variable t, is the same. Or (as in one $t = h\,\Delta$, and in the other $t = h'\,\Delta'$, the errors of the second kind being designated by Δ') there will be as many errors in proportion to the whole number in both kinds within the limits Δ and Δ', if we determine one value from the other by the equation

$$h\,\Delta = h'\,\Delta'; \qquad (11.)$$

the constants h are therefore in the inverse proportion of the equally probable errors of two kinds of observations. This is true for all errors, consequently for the probable errors of each kind, as already shown by the equation $h = \frac{\varrho}{r}$, because ϱ is here an absolute number. If, therefore, there is an even chance that an error falls in one kind within one limit, and in the other kind within the other limit, for which generally the probable errors r and r' are chosen, we have also the reciprocal proportion of the constants h and h', from the inverse proportion of the limits, or from the probable errors r and r'. Hence may be derived a preliminary estimation of the proportion of h and h'. If in two measurements of angles there is reason to fear that an error of ω'' may have been made in one as easily as an error of $m\,\omega''$ in the other, then, if h be taken for the latter, $m\,h$ must be put for the former. On account of this constant proportion between the increase of exactness and the magnitude of h, Gauss calls this constant the *measure of precision.*

The geometric representation of the curve of probability may be also applied to this consideration. Take any unity as the general measure of Δ, or of the abscissæ; then, by means of the equation

$$\phi\,\Delta = \frac{h}{\sqrt{\pi}}\,e^{-h^2\Delta^2},$$

the whole curve could at once be drawn if the value of h were known. Consequently, if we only know an ordinate belonging to a determinate Δ, the whole curve will be fully given. If the ordinate for which $\Delta = 0$ be chosen, by comparing its value with $\frac{h}{\sqrt{\pi}}$, we have at once the value of h. If the ordinate were chosen, which divides the superficial contents of the curve into two equal parts on either side of zero, we should obtain h from the abscissa belonging to this ordinate, by means of the equation $h = \frac{\varrho}{r}$. If we even knew merely the relative proportion of two ordinates which correspond to any abscissæ, Δ and Δ', then as this proportion is as $e^{-h^2\Delta^2} : e^{-h^2\Delta'^2}$, or as $1 : e^{-h^2(\Delta'^2 - \Delta^2)}$, we should be able to determine h from hence. It is most convenient to choose for the one ordinate that which corresponds to the value $\Delta = 0$. Hence follows a

proposition which will be frequently applied in the sequel, viz.:—

IV.

If the probability of an error $= 0$ *is to the probability of an error* $= \Delta$, *as* $1 : e^{-p\Delta^2}$, *then for this set of observations we must assume* $h = \sqrt{p}$.

Such a determination of h admits even of combining together observations relating to heterogeneous magnitudes, as for example angular and linear magnitudes, provided only it be possible to deduce the relative values of h in reference to the respective unities.

An actual exemplification taken from experience may perhaps serve to illustrate this subject further, by showing how very nearly the function $\phi\,\Delta$ expresses the distribution of the errors in a sufficient number of observations. It is taken from the *Fundamenta Astronomiæ*, in which Bessel has given a memorable example of the consecutive, strict, and elegant treatment of a series of observations. He determines the value of r by a direct observation of the difference of right ascension of the sun, and of one of the two stars, *a Aquilæ* and *a Canis minoris*, as derived from BRADLEY's observations, to be

$$r = 0''\cdot 2637,$$

and then compares the number of errors which ought, according to theory, to occur between the limits $0''\cdot 0$ and $0''\cdot 1$, $0''\cdot 1$ and $0''\cdot 2$, and so on (always increasing by 0·1), with the errors given by actual experience in 470 observations.

Expressed in units of r, the interval of $0''\cdot 1 = 0\cdot 3792\, r$. If, therefore, we seek in the second table the value of the integral for the different limits, we find for

0·1	. . . . 0·3792	the number	0·20186
0·2	. . . . 0·7584	,,	0·39102
0·3	. . . . 1·1376	,,	0·55705
0·4	. . . . 1·5168	,,	0·69372
0·5	. . . . 1·8960	,,	0·79904
0·6	. . . . 2·2752	,,	0·87511
0·7	. . . . 2·6544	,,	0·92661
0·8	. . . . 3·0336	,,	0·95926
0·9	. . . . 3·4128	,,	0·97866
1·0	. . . . 3·7920	,,	0·98983
	. . . . ∞	,,	1·00000

If we now deduct every number from the following one, and multiply the remainder by the number of the observations = 470, we find

Between	Number of errors.	According to theory.	According to experience.
0·0 and 0·1	0·20186	95	94
0·1 „ 0·2	0·18916	89	88
0·2 „ 0·3	0·16603	78	78
0·3 „ 0·4	0·13667	64	58
0·4 „ 0·5	0·10532	50	51
0·5 „ 0·6	0·07607	36	36
0·6 „ 0·7	0·05150	24	26
0·7 „ 0·8	0·03265	15	14
0·8 „ 0·9	0·01940	9	10
0·9 „ 1·0	0·01117	5	7
Above 1·0	0·01017	5	8

In other examples also it is found, for the most part, that the larger errors occur somewhat more frequently in experience than according to theory, a proof that the assumption of the limits $-\infty$ and $+\infty$ has not misled us; for, if it had, the contrary would have been the case. This deviation is easily explained from the circumstance, that the larger errors suppose in the rule a very unusual combination of unfavourable influences, and, indeed, are frequently occasioned by occurrences so insulated that no theory could subject them to calculation.

The determinate value of one of the constants h or r in a set of observations can, however, only be deduced from actual experience, or from a series of errors which have been found to occur in this set of observations. We must first learn how to proceed, in order to obtain in the given observations those errors which approximate most nearly to the true errors; and we must then see how the function $\phi\,\Delta$ is to be determined numerically in all its parts from those errors. We may begin with the most simple case. It will afterwards be more easy to take a view of the rules for the more general and complicated cases, as the general fundamental propositions remain unaltered.

For the value of an unknown magnitude x, let direct observation, repeated m number of times, in the same manner and under completely equal circumstances, have given m values

$$n, n', n'', n''', \&c.$$

Each insulated observation will have given an approximate value by virtue of the equations of condition,

$$x - n = 0,$$
$$x - n' = 0,$$
$$x - n'' = 0,$$

and each equation of condition is the general expression of the error of the observation in any hypothesis as to x. Consequently, if the constant h belong to this set of observations, so that for it

$$\phi(\Delta) = \frac{h}{\sqrt{\pi}} e^{-h^2 \Delta^2};$$

then the general expression for the probability of one error in the first observation will be in every assumption as to x,

$$\frac{h}{\sqrt{\pi}} e^{-h^2 (x-n)^2}$$

and the joint probability of the concurrence of m errors in these observations will be

$$= \frac{h^m}{\pi^{\frac{1}{2}m}} e^{-h^2 \left\{(x-n)^2 + (x-n')^2 + (x-n'')^2 \ldots\right\}}.$$

This probability will be greatest when the sum of the squares of the remaining errors according to an adopted hypothesis is the least possible, and consequently according to

Proposition II.—*That hypothesis as to x in which the sum of the squares of the remaining errors is an absolute minimum, is the most probable of all possible hypotheses.*

This minimum may be obtained either by the differential calculus, by which

$$2(x-n) + 2(x-n') + 2(x-n'') \ldots\ldots = 0,$$

or

$$x = \frac{n + n' + n'' + \ldots\ldots}{m};$$

thus the arithmetical mean, as was before laid down, is the most probable value in equally good observations. But when x is left undetermined, we may give to the sum of the squares of the errors such a quadratic form, that both the most probable value of x, and the remaining minimum squares of the errors, may at once proceed therefrom. For the sake of brevity we will designate the sum

$$\left.\begin{array}{lll} n + n' + n'' & \ldots\ldots & \text{by } [n] \\ n^2 + n'^2 + n''^2 & \ldots\ldots & \text{by } [n^2]. \end{array}\right. \qquad (12.)$$

This mode of designation will be extended in the sequel to any

symmetrical function of any given magnitude. The compound probability, if every error is actually squared, will be

$$\frac{h^m}{\pi^{\frac{1}{2}m}} e^{-h^2 \{m x^2 - 2[n]x + [n^2]\}},$$

to which expression it is easy to give the form

$$\frac{h^m}{\pi^{\frac{1}{2}m}} e^{-h^2 \left\{ [n^2] - \frac{[n]^2}{m} + m\left(x - \frac{[n]}{m}\right)^2 \right\}}.$$

Consequently the negative exponent will be the smallest for

$$x = \frac{[n]}{m}, \tag{13.}$$

and the minimum of the squares of the remaining errors is

$$= [n^2] - \frac{[n]^2}{m}. \tag{14.}$$

This form leads at once to the estimation of the exactness of this determination of x. If we take

$$x = \frac{[n]}{m},$$

then the probability of this hypothesis becomes

$$\frac{h^m}{\pi^{\frac{1}{2}m}} e^{-h^2 \left\{ [n^2] - \frac{[n]^2}{m} \right\}}.$$

But any other value of x,

$$x = \frac{[n]}{m} + \Delta',$$

has the probability

$$\frac{h^m}{\pi^{\frac{1}{2}m}} e^{-h^2 \left\{ [n^2] - \frac{[n^2]}{m} + m\Delta'^2 \right\}}.$$

Consequently, according to Proposition II., the probability of the arithmetical mean being the true value, is to the probability of its being erroneous by the magnitude Δ', as

$$1 : e^{-h^2 m \Delta'^2};$$

or, according to the above proposition (IV.), the value of H, which is deduced from m equal observations, and which belongs to this determination of x, is

$$\mathrm{H} = h\sqrt{m}, \tag{15.}$$

so that the function $\phi\,\Delta$ for this determination of x becomes

$$\phi\,\Delta = \frac{h\sqrt{m}}{\sqrt{\pi}} e^{-h^2 m \Delta^2}.$$

In some cases, instead of expressing the relative exactness of

two determinations by the proportions of their respective values of h and r, it is more convenient to bring in the new idea of weight. By the *weight* of a given value we understand the number of equally good observations of a determinate kind (of which the exactness is to be viewed as the unit of exactness), which are required to furnish, by their arithmetical mean, a determination of equal exactness to that of the given value. According to this, in the present case, if the weight of the single observation be regarded as unity, the weight of $x = m$;—if h be the measure of the exactness of the single observations, the measure of the exactness of $x = h \sqrt{m}$,—and if the probable error of an observation be designated by r, the probable error of $x = \frac{\rho}{\mathrm{H}} = \frac{\rho}{h \sqrt{m}} = \frac{r}{\sqrt{m}}$. The weights of two determinations are to each other in the direct proportion of the squares of their respective measures of exactness, and in the inverse proportion of the squares of the probable errors.

If we substitute in the equations of condition the most probable values of x, then the differences, between the result calculated with this value, and the actual observation, are to be regarded as the errors of observation which approximate most nearly to the truth; therefore, so long as we have no means of determining the value of x more nearly, the errors thus obtained are to be regarded as the true ones. The sum of their squares must, according to the whole deduction hitherto, be equal to the minimum just determined, or it must be $= [n^2] - \frac{[n]^2}{m}$. In order to obtain, generally, a more convenient expression for this sum, we introduce a new idea, that of the *mean error*. By *mean error* we understand the magnitude which is obtained, if the sum of the squares of the *true errors* of observation be divided by the number of observations, and the square root of the quotient be extracted. Consequently, in the present case, the mean error being designated by ε_2,

$$\varepsilon_2 = \sqrt{\left(\frac{[n^2] - \frac{[n]^2}{m}}{m}\right)};$$

or

$$m\,\varepsilon_2^2 = [n^2] - \frac{[n]^2}{m},$$

inasmuch as we are at present obliged to regard the errors resulting from the most probable hypothesis as the true ones. We

may also define the *mean error* thus: it is the error which, if it alone were assumed in all the observations indifferently, would give the same sum of the squares of the errors as that which actually exists. According to this, the probability W, of the concurrence of m true observation errors is, generally, in any hypothesis which can be made as to the constant h of the function $\phi\,\Delta$,

$$W = \frac{h^m}{\pi^{\frac{1}{2}m}} e^{-h^2 m \varepsilon_2^2}.$$

From this value we are now enabled to determine the most probable value of h; for if the m observation errors, and consequently also ε_2, have actually been found, and cannot be further altered, then the maximum of this function W will depend only on the value of h. The most probable value of h will be that which makes this function W a maximum.

We may first seek this maximum by the differential calculus. If we write the above expression thus:

$$\log. W = m \log h - \tfrac{1}{2} m \log \pi - h^2 m \varepsilon_2^2,$$

then the condition of the maximum is

$$0 = \frac{m}{h} - 2\, m\, h\, \varepsilon_2^2;$$

or

$$1 = 2\, h^2 \varepsilon_2^2,$$

whence

$$h = \frac{1}{\varepsilon_2 \sqrt{2}}.$$

We may also develope generally the magnitude W as a function of h, for altered values of h. Let the value W′ belong to a value $h + \Delta$, just as the value W belongs to h, we shall then have

$$\log W' = m \log (h + \Delta) - \tfrac{1}{2} m \log \pi - (h + \Delta)^2 m \varepsilon_2^2;$$

if we write here for $m \log (h + \Delta)$ the expression

$$m \log h + m \log \left(1 + \frac{\Delta}{h}\right)$$

and develope the latter part into the known series, we have

$$\begin{aligned} \log W' = {} & m \log h - \tfrac{1}{2} m \log \pi - h^2 m \varepsilon_2^2 \\ & + m \frac{\Delta}{h} - \tfrac{1}{2} m \frac{\Delta^2}{h^2} + \tfrac{1}{3} m \frac{\Delta^3}{h^3} - \tfrac{1}{4} m \frac{\Delta^4}{h^4} \\ & - 2\, m\, \varepsilon_2^2\, h\, \Delta - m\, \varepsilon_2^2\, \Delta^2: \end{aligned}$$

and combining with the expression of log W

$$\log\left(\frac{W'}{W}\right) = \left(\frac{m}{h} - 2\,m\,h\,\varepsilon_2^2\right)\Delta - \left(\tfrac{1}{2}\,\frac{m}{h^2} + m\,\varepsilon_2^2\right)\Delta^2$$

$$+ \tfrac{1}{3}\,\frac{m}{h^3}\,\Delta^3 - \tfrac{1}{4}\,\frac{m}{h^4}\,\Delta^4 +, \text{\&c.}$$

If the value of h is here to become the most probable (consequently if W is to be an absolute maximum, and $\log\frac{W'}{W}$ is on that account always to have a negative value) the coefficient of Δ must be made $= 0$. For the maximum of W there will be

$$\frac{m}{h} - 2\,m\,h\,\varepsilon_2^2 = 0, \quad \text{or } \frac{1}{h} = \varepsilon_2\sqrt{2}, \qquad (16.)$$

and if we substitute this most probable value in the remaining members, every other value of W, so far as it depends on another h, will be given by the formula

$$W' = W\,.\,e^{-2\,m\,\varepsilon_2^2\,\Delta^2\left\{1 - \frac{1}{3}(\varepsilon_2\sqrt{2})\,\Delta + \frac{1}{4}(\varepsilon_2\sqrt{2})^2\Delta^2\,\ldots\right\}}$$

We may here make the series contained as factor in the exponent $= 1$. For if we introduce the value of the most probable h, it becomes

$$= 1 - \tfrac{1}{3}\,\frac{\Delta}{h} + \tfrac{1}{4}\,\frac{\Delta^2}{h^2} - \tfrac{1}{5}\,\frac{\Delta^3}{h^3}\;\ldots\;.$$

which series must still be multiplied by $m\,\frac{\Delta^2}{h^2}$. If $\frac{\Delta}{h}$ is a small fraction the series will deviate little from unity, and, still more, the difference of the complete rigorous value from the approximate $e^{-m\frac{\Delta^2}{h^2}}$ will be quite insensible. But if $\frac{\Delta}{h}$ were to have a greater value, W′ would be very small compared to W, and for that reason the whole accurate expression would have no material interest. Hence the probability that $h = \frac{1}{\varepsilon_2\sqrt{2}}$ or W, is to the probability that the value of $h = \frac{1}{\varepsilon_2\sqrt{2}} + \Delta$, or W′ as

$$1 : e^{-2\,m\,\varepsilon_2^2\,\Delta^2} \quad \text{or} \quad 1 : e^{-\frac{m}{h^2}\,\Delta^2}.$$

Consequently, according to Proposition IV., the measure of precision for the value $\frac{1}{\varepsilon_2 \sqrt{2}}$ of h

$$= \varepsilon_2 \sqrt{2\, m} \quad \text{or} \quad = \frac{1}{h} \sqrt{m};$$

and the probable error of this determination

$$= \frac{\varrho\, h}{\sqrt{m}} = \frac{\varrho}{\varepsilon_2 \sqrt{2}} \cdot \frac{1}{\sqrt{m}};$$

or there is an even chance that the true value of h lies between

$$\frac{1}{\varepsilon_2 \sqrt{2}} \left\{1 + \frac{\varrho}{\sqrt{m}}\right\} \text{ and } \frac{1}{\varepsilon_2 \sqrt{2}} \left\{1 - \frac{\varrho}{\sqrt{m}}\right\}: \qquad (17.)$$

hence, inasmuch as

$$r = \frac{\varrho}{h},$$

it follows at once that the *probable error* of a single observation depends on the *mean error* by the equation

$$r = \varrho \sqrt{2} \,.\, \varepsilon_2 = 0{\cdot}674489\ \varepsilon_2 \qquad (18.)$$

if the numerical value of $\varrho \sqrt{2}$ is substituted. The certainty of this determination is given by the limiting values of h. It is an even chance that r lies between

$$\frac{\varrho \sqrt{2}}{1 + \frac{\varrho}{\sqrt{m}}}\, \varepsilon_2 \quad \text{and} \quad \frac{\varrho \sqrt{2}}{1 - \frac{\varrho}{\sqrt{m}}}\, \varepsilon_2;$$

instead of which, as absolute exactness is not contemplated, we may permit ourselves to make the limits of $r =$

$$\varepsilon_2 \,.\, \varrho \sqrt{2} \left(1 - \frac{\varrho}{\sqrt{m}}\right) \text{ and } \varepsilon_2 \,.\, \varrho \sqrt{2} \left(1 + \frac{\varrho}{\sqrt{m}}\right). \qquad (19.)$$

We neglect in this the higher powers than the first of the uncertainty of the probable error, considering the uncertainty as a small magnitude of the first order.

There still remains a circumstance to be attended to. The magnitude ε_2 and with it h also, ought properly to have been determined from the true errors of observation, whereas it has been only deduced from the obtained minimum of the squares of the errors. It is clear that this mode of deduction is necessarily somewhat faulty, as every value of x which differs ever so little from the arithmetical mean must give a greater ε_2 and a lesser h. In order to gain a clearer view of this, let the most probable value of x, as far as it follows from m observations, $= p$, or

$$p = \frac{[n]}{m},$$

but let the true value be $p + \Delta p$. By substituting p in the equations of condition, we obtain as the errors of the observations the magnitudes $p - n, p - n', p - n'' \ldots$, which for the sake of brevity may be designated by $\alpha, \alpha', \alpha''$. The substitution of the true value $p + \Delta p$ would have given $p + \Delta p - n$, $p + \Delta p - n', p + \Delta p - n'' \ldots$, and these latter magnitudes, which may be called $\delta, \delta', \delta''$, would have been the true errors of observation. We have consequently the equations

$$\alpha + \Delta p = \delta$$
$$\alpha' + \Delta p = \delta'$$
$$\alpha'' + \Delta p = \delta'', \text{ \&c.}$$

As $[\alpha] = 0$, the sum of the squares taken on both sides will give

$$[\alpha^2] + m \Delta p^2 = [\delta^2].$$

Thus if we assume $[\alpha^2]$ as the true sum of the squares of the errors, we shall always err by the positive magnitude $m \Delta p^2$. This representation gives at once the means of correcting the error as far as circumstances permit. If to the m observations a new one were added without our knowing determinately what value it had given, we should have to add to the $[\alpha^2]$ the value ε_2^2 as the mean value of such a square. The equation shows that $m \Delta p^2$ must be added in every case; and it follows from what has been said above, that p has the weight m, or that if a single observation has the mean error ε_2, the mean error of p will be equal to $\frac{\varepsilon_2}{\sqrt{m}}$. Hence it follows, that we approach the truth as nearly as possible, if in this equation we take the magnitude of Δp such as its proportion to the single observations gives it, or if we substitute the value $\Delta p = \frac{\varepsilon_2}{\sqrt{m}}$. Then

$$[\alpha^2] + \varepsilon_2^2 = [\delta^2],$$

and as it follows from the assumed definition that

$$[\delta^2] = m \varepsilon_2^2$$

the value of ε_2 derived from the m errors remaining over after the substitution of the arithmetical mean is obtained by

$$(m - 1) \cdot \varepsilon_2^2 = [\alpha^2]. \tag{20.}$$

In order to obtain as nearly as possible the true mean errors of the observations, we must, with an unknown magnitude, regard the sum of the squares of the errors as if it belonged not to m, but to $(m-1)$ errors.

We may convince ourselves also of the general correctness of this rule in the following way; at least we may do so in a preliminary manner. If μ unknown magnitudes are to be found, μ equations of condition independent of each other are in every case requisite; and if no more than μ such equations are given, we have no remaining standard for the estimation of the possible error. We do not obtain this until we substitute in other equations of condition the values found instead of the unknown magnitudes, and compare the errors which result; so that with m observations treated in this manner there result $m-\mu$ errors, which allow us to form a judgement as to the exactness. Inasmuch as we do not regard the μ determinate equations alone as the absolutely correct ones, and the deviations of all other results from those drawn from the μ chosen results as errors, but as we give to all an equal share in determining the unknown magnitudes, we are certainly nearer the truth; but we do not by this means get rid of the analytical necessity of always applying to the determination of μ unknown magnitudes, if not μ determinate equations, yet an equivalent to such μ equations taken from all together. Consequently the functions of the remaining errors thus obtained will always refer, not to a number of m errors, but to the number of $m-\mu$ errors, as has been shown for $\mu = 1$, and as will be shown in the sequel for any μ taken at pleasure.

A view of the rules for the heretofore considered simplest case, *i. e.* the case of equally good direct observations of an unknown quantity,—may be facilitated by applying them to BENZENBERG's latest and most exact experiments on the fall of bodies, made in the Schlebuscher coal mines. The object of these experiments was to demonstrate directly the rotation of the earth round its axis, by showing that balls let fall from a considerable height without initial velocity, deviate, in falling to the lower station, towards the east, more than a plumb-line suspended from the same upper point. The experiments, although divided into separate parts, were so made as all to have the same value. As they are only used as an example, I leave quite out of consideration the deviation (not agreeing with theory) of single balls towards the north and south, which moreover almost entirely disappeared in the mean of all the experiments. I also take as valid only those experiments which the observer himself selects,—Table, page 424, '*Versuche*

über das Gesetz des Falles, &c.,' by J. F. BENZENBERG, Dortmund, 1804,—although the reasons given for the exclusion of others are perhaps not quite convincing. Designating the easterly deviation from the perpendicular by +, and the westerly by −, the following deviations in Parisian lines were observed in a height of 262 Parisian feet.

	n		n
	'''		'''
Experiment 1.	− 3·0	Experiment 16.	− 8·0
" 2.	+12·0	" 17.	+ 8·0
" 3.	+ 3·0	" 18.	+10·0
" 4.	+13·0	" 19.	+ 7·0
" 5.	+20·0	" 20.	+ 7·5
" 6.	− 2·0	" 21.	+ 6·0
" 7.	+11·5	" 22.	− 2·0
" 8.	− 4·0	" 23.	+11·0
" 9.	+ 2·0	" 24.	− 4·0
" 10.	+ 2·0	" 25.	− 9·0
" 11.	+12·0	" 26.	−10·0
" 12.	+ 7·0	" 27.	+ 8·5
" 13.	+13·5	" 28.	+10·0
" 14.	+11·0	" 29.	+ 5·5
" 15.	+ 9·0		

If x designates the deviation sought, the simple form of the equations of condition is here

$$x - n = 0;$$

consequently, according to (13.) the most probable deviation is

$$x = \frac{+\,189{\cdot}5 - 42{\cdot}0}{29} = +\,5'''{\cdot}086,$$

and the errors remaining over, arranged according to their absolute magnitudes, are,

	'''		'''
Experiment 29.	−0·414	Experiment 7.	−6·414
" 21.	−0·914	" 2.	−6·914
" 12.	−1·914	" 11.	−6·914
" 19.	−1·914	" 6.	+7·086
" 3.	+2·086	" 22.	+7·086
" 20.	−2·414	" 4.	−7·914
" 17.	−2·914	" 1.	−8·086
" 9.	+3·086	" 13.	−8·414

Experiment	10.	$+3'''.086$	Experiment	8.	$+\ 9'''.086$
"	27.	-3.414	"	24.	$+\ 9.086$
"	15.	-3.914	"	16.	$+13.086$
"	18.	-4.914	"	25.	$+14.086$
"	28.	-4.914	"	5.	$+14.914$
"	14.	-5.914	"	26.	$+15.086$
"	23.	-5.914			

The sum of the squares of these errors will be found, either by immediately squaring each single error, or by means of formula (14.) to be

$$= 1612{\cdot}0; \quad \text{also } m = 29;$$

consequently,

$$\varepsilon_2 = \sqrt{\frac{1612{\cdot}0}{28}} = 7'''{\cdot}588,$$

whence

$$r = \varepsilon_2 \,.\, \varrho \sqrt{2} = 5'''{\cdot}118,$$

and

$$h = \frac{\varrho}{r} = 0{\cdot}093,$$

the unit being the Parisian line.

As $m=29$, and therefore $\frac{\varrho}{\sqrt{m}} = 0{\cdot}08846$, it is an equal chance that

ε_2 .. will be between $6'''{\cdot}916$ and $8'''{\cdot}260$,
r... " $4\ {\cdot}665$ " $5\ {\cdot}571$,
h... " $0\ {\cdot}085$ " $0\ {\cdot}101$.

Lastly, the most probable deviation, in reference to a single one of these experiments, has the weight 29; consequently its probable error (and in like manner the H belonging to it and the mean error)

$$= \frac{r}{\sqrt{29}} = 0'''{\cdot}950,$$

the limits of certainty of which are given in the same manner from the limits of r, and it is an equal chance that the true deviation lies between

$$4'''{\cdot}136 \text{ and } 6'''{\cdot}036.$$

The value given by theory, $4'''{\cdot}6$, is within these limits; therefore the experiments agree with it. They also agree sufficiently well for their small number with the value of r, according to which half the errors should be less than $5'''{\cdot}118$. Of twenty-nine errors thirteen are less than this amount, and sixteen

exceed it. If there were no easterly deviation, there would be in x an error of $5'''\cdot 086$; but as this is more than five times the probable error of x, the existence of an easterly deviation borders closely on certainty. If it were desired to determine the absolute value within narrower limits, it would be necessary to make a considerably larger number of experiments of the same kind. About 2600 would be required in order to reduce probable error of x to $0'''\cdot 1$.

It must not be overlooked, that the limit of error is clearly much too narrow, partly because with the absolute smallness of x a constant error in the kind of observation would have proportionally very great influence, partly because the exclusion of observations which deviate more than two inches (of which there were in all eleven in forty) can hardly be perfectly justified. Such an exclusion, moreover, if made after the result, is open to the danger of leading away from the pure truth, and it always produces an erroneous representation of the certainty of the result.

The most troublesome part of the calculation in this simplest case being the determination of the sum of the squares of the errors, we may wish to attain in a simpler manner to the knowledge of r and h. This examination is besides useful, as the subject is considered in it from another quarter, and the determination of h, from the sum of the squares of the errors, is attained by another way.

If the law of the errors were given generally by $\psi\ \Delta$ (without determinate assumption of the above function $\phi\ \Delta$), and if this function were completely known, then, in respect to m observations of any kind, we should be able, even before we knew their result, to form a conclusion as to the distribution of the errors and as to the magnitude of any of their functions; which conclusion would be so much the more confirmed after the observations were made, as m is greater. So for example, according to probability, there would be between $\Delta = a$ and $\Delta = b$ a number of errors

$$= m \int_a^b \psi(\Delta)\, d\,\Delta;$$

also as $m\ \psi\ (\Delta)$ is the number of errors of the magnitude Δ, the magnitude $m\ \Delta^n\ \psi\ (\Delta)$ will be the sum of the nth powers of the errors of the magnitude Δ in m observations; and, consequently,

$$m \int_{\Delta=-\infty}^{\Delta=+\infty} \Delta^n \, \psi(\Delta) \, d\Delta = m \, k^{(n)}$$

will express generally the sum of the nth powers of all the errors which, according to the law of probabilities, should occur in m observations. The magnitude $k^{(n)}$, in which the index n depends on the power of Δ, or the integral taken between the widest limits, cannot be merely an absolute number, but will contain one or more constants, having respect to the class of observations. If, therefore, we knew truly the form of $\psi(\Delta)$, but were still uncertain of the exact value of the constants contained in it, then any number of m observations, when the pure errors of observation are found thereby, would lead us to the knowledge of the constants. For, let the errors $\alpha, \beta, \gamma, \delta$, be given immediately up to the number m, then the most probable value of $k^{(n)}$ will be found by

$$k^{(n)} = \frac{\alpha^n + \beta^n + \gamma^n + \delta^n \ldots}{m} = \frac{[\Delta^n]}{m}.$$

Any other hypothesis as to the value of $k^{(n)}$ would not suppose the errors distributed according to the law $\psi(\Delta)$; consequently, it would assume an error in one or several values of α^n, β^n, γ^n, &c. The value of $k^{(n)}$, which, in its conditions, involves no error, must be the most probable, according to these m observations.

This form gives also at the same time the limits of certainty of the determination thus obtained of $k^{(n)}$. With $k^{(n)}$ the principle of the arithmetical mean holds rigorously good, by which, for each m, we find, from the results given by the observations singly, the most probable value of one and the same unknown magnitude. The magnitudes α^n, β^n, γ^n, come consequently into the series of direct observations of the magnitude $k^{(n)}$, and the differences $k^{(n)} - \alpha^n$, $k^{(n)} - \beta^n$, $k^{(n)} - \gamma^{(n)}$, are to be viewed as the errors of one such single determination. The above-determined form $\phi(\Delta)$ holds good for them, apart from the original form $\psi(\Delta)$ in every case. Hence the mean deviation of such a single determination

$$= \sqrt{\left(\frac{(k^{(n)} - \alpha^n)^2 + (k^{(n)} - \beta^n)^2 + (k^{(n)} - \gamma^n)^2 + \ldots}{m}\right)},$$

instead of which, by substitution of

$$[\Delta^n] = \alpha^n + \beta^n + \gamma^n + \delta^n \ldots = m\, k^{(n)},$$
$$[\Delta^{2n}] = \alpha^{2n} + \beta^{2n} + \gamma^{2n} + \delta^{2n} \ldots = m\, k^{2n},$$

squaring, we may write,

$$\sqrt{\{k^{2n} - k^{(n)}\ k^{(n)}\}},$$

the probable deviation of a single datum is

$$= \rho \sqrt{\{2\,(k^{(2n)} - k^{(n)}\ k^{(n)})\}},$$

and consequently that of the arithmetical mean of m data

$$= \varrho \sqrt{\frac{2\ (k^{2n}) - k^{(n)}\ k^{(n)})}{m}};$$

consequently it is an equal chance that k^n lies between

$$\frac{[\Delta^n]}{m} + \varrho \sqrt{\left(\frac{2\ (k^{(2n)} - k^{(n)}\, k^{(n)})}{m}\right)},$$

$$\text{and } \frac{[\Delta^n]}{m} - \varrho \sqrt{\left(\frac{2\ (k^{(2n)} - k^{(n)}\ k^{(n)})}{m}\right)};$$

or that

$$k^{(n)} = \frac{[\Delta^n]}{m} \left\{ 1 \pm \varrho \sqrt{\frac{2}{m}} \cdot \sqrt{\left(\frac{k^{(2\,n)}}{k^{(n)}\ k^{(n)}} - 1\right)} \right\},$$

where the bracket refers to the limiting values, between which the probability $=$.

In the application to the law $\phi\ \Delta$ found above for $\psi\ (\Delta)$, we require each time the value of $\sqrt[n]{\ }\ k^{(n)}$. Thus, if we designate generally

$$\sqrt[n]{\frac{[\Delta^n]}{m}} = \varepsilon_n,$$

and extract on both sides the nth root, neglecting the higher powers of the limiting values, then

$$\sqrt[n]{\ }\ k^{(n)} = \varepsilon_n \left\{ 1 \pm \frac{\varrho}{n} \sqrt{\frac{2}{m}} \sqrt{\left(\frac{k^{(2\,n)}}{^{(n)}\ k^{(n)}} - 1\right)} \right\}.$$

This formula requires besides only the determination of the values of $k^{(n)}$ for any n that may be taken. For the function $\phi\ \Delta$ which here obtains, we have

$$k^{(n)} = \frac{h}{\sqrt{\pi}} \int_{-\infty}^{+\infty} \Delta^n\, e^{-h^2\Delta^2}\, d\,\Delta\,;$$

or if, in order to be able to bring into calculation the uneven powers of the errors (which else must always destroy each other), we regard all errors as positive

$$k^{(n)} = \frac{2\,h}{\sqrt{\pi}} \int_0^\infty \Delta^n\, e^{-h^2 \Delta^2}\, d\,\Delta,$$

because the errors are distributed equally on both sides of 0. If we put here

$$h\,\Delta = t,$$

then

$$k^{(n)}\,\frac{h^n \sqrt{\pi}}{2} = \int_0^\infty t^n\, e^{-t^2}\, d\,t.$$

By partial integration we find the general integral

$$= -\tfrac{1}{2}\, t^{n-1}\;\; e^{-t^2} + \frac{n-1}{2} \int t^{n-2}\;\; e^{-t^2}\, d\,t.$$

The first part disappears for the limit 0 as well as ∞, because with the latter $e^{-t^2} = \frac{1}{e^{t^2}}$ will, in the development of the series, always produce higher powers of t in the denominator than those which are in the numerator; consequently,

$$k^{(n)}\,\frac{h^n \sqrt{\pi}}{2} = \frac{n-1}{2} \int_0^\infty t^{n-2}\, e^{-t^2}\, d\,t$$

$$= \frac{n-1}{2}\; k^{(n-2)}\;\; \frac{h^{n-2} \sqrt{\pi}}{2};$$

or

$$k^{(n)} = \frac{\frac{1}{2}\,(n-1)}{h^2}\, k^{(n-2)}; \quad k^{(n+2)} = \frac{\frac{1}{2}\,(n+1)}{h^2}\, k^{(n)}.$$

By the continuation of this operation we shall arrive, according as n is even or odd, either to $k^{(0)}$ or to $k^{(1)}$; but the former is, according to (5.),

$$k^{(0)} = 1;$$

and for the latter we find, by a glance at the formula

$$k^{(1)} = \frac{1}{h\sqrt{\pi}}.$$

Hence we have at once the following values:

$$k^{(0)} = 1, \qquad\qquad k^{(1)} = \frac{1}{h\sqrt{\pi}},$$

$$k^{(2)} = \frac{1}{2\,h^2}, \qquad\qquad k^{(3)} = \frac{1}{h^3 \sqrt{\pi}},$$

$$k^{(4)} = \frac{3}{4\,h^4}, \qquad\qquad k^{(5)} = \frac{2}{h^5 \sqrt{\pi}},$$

$$k^{(6)} = \frac{3.5}{8\,h^6}, \qquad\qquad k^{(7)} = \frac{2\,.\,3}{h^7 \sqrt{\pi}},$$

$$k^{(8)} = \frac{3\,.\,5\,.\,7}{16\,h^8}, \qquad\qquad k^{(9)} = \frac{2\,.\,3\,.\,4}{h^9 \sqrt{\pi}}.$$

If these values be substituted in the above formula, we have on the left side of the equation $\sqrt[n]{k^{(n)}}$

$$\text{for } n \text{ even} = \frac{1}{h} \cdot \sqrt[n]{\frac{1.3.5\ldots.(n-1)}{2.2.2\ldots.2}},$$

$$n \text{ odd} = \frac{1}{h} \cdot \sqrt[n]{\frac{1.2.3\ldots\frac{1}{2}(n-1)}{\sqrt{\pi}}}$$

consequently, if we multiply both sides by ϱ, and then leave on the left side $\frac{\varrho}{h} = r$ standing alone, we then obtain the following values:

$$r = \varrho \sqrt{\pi} \quad . \, \varepsilon_1 \left\{1 \pm \frac{\varrho}{\sqrt{m}} \sqrt{(\pi - 2)}\right\}$$

$$r = \varrho \sqrt{2} \quad . \, \varepsilon_2 \left\{1 \pm \frac{\varrho}{\sqrt{m}}\right\}$$

$$r = \varrho \sqrt[6]{\pi} \quad . \, \varepsilon_3 \left\{1 \pm \frac{\varrho}{\sqrt{m}} \sqrt{\frac{15\pi - 8}{36}}\right\}$$

$$r = \varrho \sqrt[4]{\tfrac{4}{3}} \quad . \, \varepsilon_4 \left\{1 \pm \frac{\varrho}{\sqrt{m}} \sqrt{\frac{4}{3}}\right\}$$

$$r = \varrho \sqrt[10]{\tfrac{1}{4}\pi} \, . \, \varepsilon_5 \left\{1 \pm \frac{\varrho}{\sqrt{m}} \sqrt{\frac{945\pi - 128}{1600}}\right\}$$

$$r = \varrho \sqrt[6]{\tfrac{8}{15}} \, . \, \varepsilon_6 \left\{1 \pm \frac{\varrho}{\sqrt{m}} \sqrt{\frac{113}{45}}\right\}$$

or in numbers,—

$$r = 0{\cdot}845347 \, . \, \varepsilon_1 \left\{1 \pm \frac{0{\cdot}509584}{\sqrt{m}}\right\}$$

$$r = 0{\cdot}674489 \, . \, \varepsilon_2 \left\{1 \pm \frac{0{\cdot}476936}{\sqrt{m}}\right\}$$

$$r = 0{\cdot}577190 \, . \, \varepsilon_3 \left\{1 \pm \frac{0{\cdot}497199}{\sqrt{m}}\right\}$$

$$r = 0{\cdot}512502 \, . \, \varepsilon_4 \left\{1 \pm \frac{0{\cdot}550719}{\sqrt{m}}\right\}$$

$$r = 0{\cdot}465553 \, . \, \varepsilon_5 \left\{1 \pm \frac{0{\cdot}635508}{\sqrt{m}}\right\}$$

$$r = 0{\cdot}429497 \, . \, \varepsilon_6 \left\{1 \pm \frac{0{\cdot}755776}{\sqrt{m}}\right\}$$

where ε is the arithmetical mean of all the errors without regarding their signs; ε_2 is the square root of the arithmetical

mean of the squares of the errors; and, generally, ε_n is the nth root of the arithmetical mean of the nth powers, without regard to signs.

We see from the numerical part of the limiting values, that the determination by the sum of the squares is the most advantageous one. With an equal number of observations, we obtain by its means the narrowest limits within which there are equal chances that r lies. The number of observations necessary for attaining equal limits, according as we employ ε_1, ε_2, ε_3, &c., will be to each other as

$$\pi - 2 : 1 : \frac{15\pi - 8}{36} : \frac{4}{3} : \frac{945\pi - 128}{1600} : \frac{113}{45};$$

or if with ε_2 one hundred observations are required to attain certain limits, there are required for the same limits, with

ε_1	114	observations
ε_3	109	"
ε_4	133	"
ε_5	178	"
ε_6	251	"

On account of the great convenience of ε_1, and the not very considerable difference in the narrowness of the limits, the employment of ε_1 will be most frequently preferred if the sum of the squares of the errors is not already known.

For the above example the sum of the absolute errors $= 181{\cdot}898$; consequently,

$$\varepsilon_1 = \frac{181{\cdot}898}{28} = 6'''{\cdot}496,$$

and thence

$$r = 5'''{\cdot}492$$

within the limits

$$4'''{\cdot}972 \text{ and } 6'''{\cdot}012,$$

a value which, if it differs from that above given, still leads, for the small number of observations, to a sufficient estimation of the accuracy of the result.

We may employ besides for this determination the proposition which has no direct reference to the magnitude of the single errors, but only declares that, according to the universal law of probability [without determinate assumption of $\phi(\Delta)$], the idea of the probable error contains the condition that there occur as many errors less than r, as there are greater. If, therefore, we arrange the errors, without regard to their

signs, according to their absolute magnitude, and begin to reckon from the smallest, then the error which belongs to the index $\frac{1}{2}(m+1)$, if m is an odd number,—or the arithmetical mean between the errors of which the indices are $\frac{1}{2}m$, and $\frac{1}{2}m+1$, if m is an even number,—will give an approximate value for r. In the example given above, m being $= 29$, it would be the 15th, or we should find

$$r = 5'''\cdot 914.$$

As in the sums of the powers a greater number of errors so greatly increases exactness in respect to the probable limits, the effect must be so much the greater in this mode of computing. As the necessary formula has been given without proof, by GAUSS, in the *Zeitschrift für Astronomie*, vol. i. p. 195, the following elegant demonstration, for which I am indebted to my respected colleague, Professor DIRICHLET, will have the more value, as the proposition has not yet been demonstrated elsewhere.

Let us seek the probability that, with $(2n+1)$ observations, the distribution of the errors shall be such, that there shall be *one* error between t and $t+dt$, n errors between 0 and t, and n errors between $t+dt$ and ∞. Let the probability that there is one error less than t be generally

$$= \int_0^t \psi(\Delta)\, d\,\Delta = u;$$

then the probability of an error $> t + dt$ will be

$$1 - \psi\, t\, dt - \int_0^t \psi\,\Delta\, d\,\Delta = 1 - u - \psi\, t\, dt,$$

as the probability of an error between t and $t + dt = \psi(t)\,dt$. Hence the compound probability of an arrangement of errors in which n errors $< t$, one error between t and $t+dt$, and n errors $> t + dt$.

$$= u^n\,(1-u)^n \cdot \psi(t)\, dt,$$

neglecting the members of the second order, as the result is of the first order. But there may be as many such cases or arrangements as there are possible transpositions of $2n+1$ elements, if there occur among them n equal elements of one kind (of which the probability $= u$), and n equal elements of another kind (of which the probability $= (1-u)$). Consequently the probability of all possible arrangements of this kind

$$= \frac{1.2.3\ldots(2n+1)}{(1.2.3\ldots n)^2}\; u^n\,(1-u)^n\,\psi(t)\,dt = \mathrm{U}.$$

If we regard the magnitude $d\,t$ of the interval between t and $t + d\,t$ as constant, there is a value of t, for which U is a maximum. The equation obtained by differentiation for its determination is

$$\frac{n\,\psi\,(t)}{u} - \frac{n\,\psi\,(t)}{1-u} + \psi'\,t = 0,$$

where $\psi'\,t$ has the same signification as $\phi'\,\Delta$ above. $d\,u$, or the increment of $\int_0^t \psi\,\Delta\,d\,\Delta$, in reference to an infinitely small alteration of the limit t, is equal to $\psi\,(t)\,d\,t$. We may give to the last equation the form

$$\frac{1}{u} - \frac{1}{1-u} + \frac{\psi' t}{n\,\psi\,t} = 0.$$

The last member will be so much the less as n is greater, or as there are more observations given. With a sufficiently large number it may be neglected. Or as n increases, the value of t, for which the maximum takes place, approximates continually to the value which follows from the equation

$$\frac{1}{u} - \frac{1}{1-u} = 0,$$

whence

$$u = \int_0^t \psi\,(\Delta)\,d\,\Delta = \tfrac{1}{2},$$

or the value of r, according to the definition given above.

If we take the integral of U between determinate limits, we may obtain the probability that the error which is situated in the middle is contained in these limits. It will be for the limits $r - \delta$ and $r + \delta$

$$= \frac{1\,.\,2\,.\,3\,\ldots\,(2\,n+1)}{(1\,.\,2\,.\,3\,\ldots\,n)^2} \int_{r-\delta}^{r+\delta} u^n\,(1-u)^n\,\psi\,(t)\,d\,t,$$

or because $\psi\,(t)\;d\,t = d\,u$, if for the limits in relation to t we put

$$\int_0^{r-\delta} \psi\,(t)\;d\,t = u', \int_0^{r+\delta} \psi\,(t)\;d\,t = u'',$$

then the probability that the middlemost value lies between $r - \delta$ and $r + \delta$ will be

$$= \frac{1\,.\,2\,.\,3\,\ldots\,(2\,n+1)}{(1\,.\,2\,.\,3\,\ldots\,n)^2} \int_{u'}^{u''} u^n\,(1-u)^n\,d\,u.$$

The greater the number of observations, the narrower will be the limits between which t will lie with equal probability. Therefore, if the observations are sufficiently numerous, developing u' and u'' according to TAYLOR'S theorem, we may be permitted to consider only the first power of δ. Whence,

$$u' = \int_0^r \psi\, t\, d t - \delta \psi\ (r)\ \ldots = \tfrac{1}{2} - \delta \psi\, (r)\,;$$

and similarly,

$$u'' = \tfrac{1}{2} + \delta \psi\, (r).$$

This form, as well as the combination of u and $1 - u$ in the integral, shows that a still more convenient form may be obtained by bringing in another variable for u; and this may be best done by the equation

$$u = \tfrac{1}{2} + \frac{s}{2\sqrt{n}} = \tfrac{1}{2} \left(1 + \frac{s}{\sqrt{n}}\right)$$

consequently,

$$1 - u = \tfrac{1}{2} - \frac{s}{2\sqrt{n}} = \tfrac{1}{2} \left(1 - \frac{s}{\sqrt{n}}\right),$$

the limits in relation to s being found by

$$\delta \psi\, (r) = \frac{s}{2\sqrt{n}}.$$

According to this the integral becomes

$$\frac{1\,.\,2\,.\,3\ldots(2\,n+1)}{(1\,.\,2\,.\,3\ldots n)^2}\,.\,\frac{1}{2^{2n+1}\sqrt{n}}\int_{-2\,\delta\sqrt{n}\,\psi\,(r)}^{+2\,\delta\sqrt{n}\,\psi\,(r)} \left(1 - \frac{s^2}{n}\right)^n d\,s.$$

or, because s in the differential contains only even powers,

$$\frac{1\,.\,2\,.\,3\ldots(2\,n+1)}{(1\,.\,2\,.\,3\ldots n)^2}\quad\frac{1}{2^{2n}\sqrt{n}}\int_0^{2\,\delta\sqrt{n}\,\psi\,(r)} \left(1 - \frac{s^2}{n}\right)^n d\,s.$$

Now let $\delta\sqrt{n}$ be a finite magnitude $= \gamma$; thus the limit δ decreasing with the increase of $\sqrt{n}$, s remains finite within the assumed limits, however much n may increase. But if n is large, we may, according to the development of logarithms in EULER'S *Introductio*, put

$$\left(1 - \frac{s^2}{n}\right)^n = e^{-s^2};$$

and

$$\frac{1.2.3\ldots 2n}{(1.2.3\ldots n)^2} = \frac{2^{2n}}{\sqrt{n\pi}},$$

EULER *Calc. Diff.* P. ii. Cap. vi. § 160–162, as the limiting value to which it continually approximates as n increases; so that the expression becomes

$$\frac{2n+1}{n\sqrt{\pi}} \int_0^{2\delta\sqrt{n}\,\psi(r)} e^{-s^2}\,ds;$$

for which we need not scruple to write

$$\frac{2}{\sqrt{\pi}} \int_0^{2\delta\sqrt{n}\,\psi(r)} e^{-s^2}\,ds,$$

as the expression of the probability that with numerous observations, the middlemost error, all being arranged according to their magnitudes, lies between $r - \delta$ and $r + \delta$. This probability consequently becomes $\frac{1}{2}$, or the probable limits are given by

$$2\delta\sqrt{n}\,\psi(r) = \varrho,$$

whence

$$\delta = \frac{\varrho}{2\sqrt{n}} \cdot \frac{1}{\psi(r)}.$$

For the law of the errors assumed above

$$\psi(\Delta) = 2\,\phi(\Delta) = \frac{2h}{\sqrt{\pi}}\, e^{-h^2\Delta^2}$$

the probable limits of r will be

$$r \pm \frac{\varrho\, e^{h^2 r^2}\sqrt{\pi}}{4\sqrt{n}\,.\,h}$$

or if, instead of $2n + 1$, we call the number of observations m, and if we employ the equation $hr = \varrho$,

$$r\left\{1 \pm \frac{e^{\varrho^2}\sqrt{\pi}}{\sqrt{(8m)}}\right\}.$$

The numerical value of e^{ϱ^2} is 1·2554176, whereby the expression becomes

$$r\left\{1 \pm \frac{0{\cdot}786716}{\sqrt{m}}\right\}.$$

This mode of determination of r is consequently still more inexact

than any one of the former ones up to the sum of the 6th powers. Applied to the above example, r becomes

$$r = 5'''\cdot914 \pm 0'''\cdot864,$$

or the limits

$$5'''\cdot050 \quad \text{and} \quad 6'''\cdot778.$$

In the demonstrations hitherto given, it has frequently been necessary to conclude from the probability of one value to that of another value depending on that of the first in a simple manner. For the sequel it becomes necessary to solve the general problem. If we know the most probable values of certain independent magnitudes x, x', x'', &c., and the different limits within which these most probable values will lie, if any determinate probability is to be ascribed to them, to determine the most probable value of any function of these variables,

$$X = f\,(x, x', x'', \ldots),$$

and also the limits within which X has the same determinate probability. As, when we know the value of r in a magnitude deduced by observations, we can at once find h, ε_2, and all the other functions of the errors, as well as their complete law $\phi\,(\Delta)$, the problem may be proposed in this way: for x, x', x'', the most probable values a, a', a'' having been found independently of each other with the probable errors r, r', r'' ...., it is required to determine the most probable value of $X = f(x, x', x''\ldots)$ and its probable error.

To begin with the simplest case, let X be a linear function of *one* unknown quantity

$$X = \alpha\, x.$$

In all the cases in which $x = a$, $X = \alpha\, a$, consequently this will also be the most probable value of X. So also the cases in which x lies between $a-r$ and $a+r$ are equal in number to the cases in which X lies between $\alpha\, a - \alpha\, r$, and $\alpha\, a + \alpha\, r$; or

$$X = \alpha\, a \pm \alpha\, r,$$

where the last member denotes the probable error of X.

Now, in the second place, let X be the simple linear function of *two* variables

$$X = x + x'.$$

For the sake of more convenient expression, let us now introduce, in lieu of the probable error, the weight of the values a and a'. If an observation, of which the probable error is w, be taken

as a common standard, then the weight of a, by reason of its probable error r, will be

$$p = \frac{w^2}{r^2},$$

and likewise for a'

$$p' = \frac{w^2}{r'^2}.$$

Hence, if h belongs to w, the probability of any value for x

$$= \frac{h\sqrt{p}}{\sqrt{\pi}}\, e^{-h^2 p (x-a)^2},$$

and for x'

$$= \frac{h\sqrt{p'}}{\sqrt{\pi}}\, e^{-h^2 p' (x'-a')^2};$$

thus the probability of the concurrence of two arbitrary values will be

$$\frac{h^2\sqrt{pp'}}{\pi}\, e^{-h^2\{p(x-a)^2 + p'(x'-a')^2\}};$$

and the probability of the concurrence of two values x and x' which satisfy the equation

$$x + x' = \mathrm{X},$$

in which X signifies an arbitrary but determinate value, is found by considering one of the magnitudes x or x' as a function of the other, and of the magnitude X, and by substituting the value so obtained. Hence the probability that any value x, by its concurrence with the value x', should give the result X, is

$$\mathrm{W} = \frac{h^2\sqrt{pp'}}{\pi}\, e^{-h^2\{p(x-a)^2 + p'(\mathrm{X}-x-a')^2\}}.$$

Then, if we take the sums of all possible values of W, or the $\int \mathrm{W}\, dx$ within the limits in which a value of x can exist, being here $-\infty$ and $+\infty$, we shall have embraced all cases in which X can be obtained, or have determined the probability of X. In order to facilitate the integration, let us give to the exponents the following form:

$$-h^2\left\{(p+p')\left(x - \frac{p'\mathrm{X} + pa - p'a'}{p+p'}\right)^2 + \frac{pp'}{p+p'}(\mathrm{X}-a-a')^2\right\},$$

which is at once obtained if we combine in a quadratic form all

the members which contain x. Now, for temporary abbreviation, let

$$x - \frac{p' X + pa - p' a'}{p + p'} = x_0,$$

$$X - a - a' = X_0;$$

then

$$\int_{-\infty}^{+\infty} W\, d\, x = \frac{h}{\sqrt{\pi}} \sqrt{\left(\frac{p p'}{p + p'}\right)} \cdot e^{-h^2 \frac{p p'}{p + p'} X_0^2}$$

$$\times \frac{h \sqrt{(p + p')}}{\sqrt{\pi}} \int_{-\infty}^{+\infty} e^{-h^2 (p + p') x_0^2}\, d\, x_0,$$

the value of the factor which contains the integral will, according to (5), $= 1$; consequently the probability of X

$$= \frac{h}{\sqrt{\pi}} \sqrt{\left(\frac{p p'}{p + p'}\right)} \cdot e^{-h^2 \frac{p p'}{p + p'} (X - a - a')^2}$$

is a maximum, if

$$X = a + a',$$

and the weight of this determination will be given immediately by the form

$$P = \frac{p p'}{p + p'};$$

consequently the probable error

$$= \frac{w}{\sqrt{P}} = w \sqrt{\frac{p + p'}{p p'}} = \sqrt{\left(\frac{w^2}{p'} + \frac{w^2}{p}\right)}$$

$$= \sqrt{(r^2 + r'^2)}.$$

The simple proposition thus found is this: If a and a', the most probable values of x and x', independently of each other, together with the probable errors r and r', are given, then the most probable value of $X = x + x'$

$$= a + a',$$

and the probable error of this value

$$= \sqrt{(r^2 + r'^2)}.$$

Combining this with the preceding proposition, we obtain consequently for any linear function

$$\left.\begin{array}{l} X = \alpha\, x + \beta\, x' + \gamma\, x'' \ldots. \\ \text{the most probable value} \\ \quad = \alpha\, a + \beta\, a' + \gamma\, a'' \ldots. \\ \text{with the probable error} \\ \quad = \sqrt{(\alpha^2 r^2 + \beta^2 r'^2 + \gamma^2 r''^2 \ldots.)} \end{array}\right\} \quad . \quad . \quad (20.)$$

because, by virtue of the form for two unknown magnitudes, the form for any number is at once derived,—if there are three, by first combining two with each other, and then combining their result with the third,—if there are four, by first combining three with each other, and then their result with the fourth, and so on.

The general problem might be solved in a similar manner if the integrations could be performed. For

$$X = f(x, x', x'' \ldots.) \quad . \quad . \quad . \quad . \quad . \quad (21.)$$

the probability of the concurrence of arbitrary values of the μ variables will be

$$= \frac{h^{\mu} \sqrt{(p.p'.p'' \ldots)}}{\pi} \, e^{-h^2 (p(x-a)^2 + p'(x'-a')^2 + p''(x''-a'')^2 \ldots)}.$$

If we are here to consider only the cases in which a determinate value for X is to be found, let us express one of the variables x as a function of X and the remainder. If we substitute this value in the exponents, and take the sums or integrals within all possible limits for $x, x' \ldots$, we shall obtain the probability of the value X, and we may thence determine the most probable value and its limits. But for this the knowledge of the function is obviously needed; and, if this function is not linear, the complete integration will be impracticable in most cases. However, under the supposition that the limits for the several variables are already so narrow, that the higher powers of the probable error may be neglected, we may find an approximative value for X and its limits, which will be always sufficient in practice.

Let us take for arbitrary values of $x, x', x'' \ldots$ the form $a + \Delta x, a' + \Delta x', a'' + \Delta x''$; then, if

$$V = f(a, a', a'' \ldots) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (22.)$$

the general expression for X, neglecting those powers of Δx, $\Delta x'$, $\Delta x''$, which exceed the first, will be

$$X = V + \left(\frac{dV}{da}\right) \Delta x + \left(\frac{dV}{da'}\right) \Delta x' + \left(\frac{dV}{da''}\right) \Delta x'' \ldots,$$

or

$$X - V = \left(\frac{dV}{da}\right) \Delta x + \left(\frac{dV}{da'}\right) \Delta x' + \left(\frac{dV}{da''}\right) \Delta x'' \ldots,$$

and the probability of the concurrence of these values will be

$$= \frac{h^{\mu} \sqrt{p.p'.p'' \ldots}}{\pi^{\frac{1}{2}\mu}} \, e^{-h^2 (p \Delta x^2 + p' \Delta x'^2 + p'' \Delta x''^2 \ldots)}.$$

The most probable value of $X - V$ and its limits are given immediately by the most probable value of X and its limits, and conversely, because the two magnitudes $X - V$ and X differ only by a constant; so also the probable errors of Δx, $\Delta x'$, $\Delta x''$, &c. will be the given magnitudes r, r', r'', &c. and the most probable values of Δx, $\Delta x'$, $\Delta x''$, &c. will be nothing by virtue of the equations $x = a + \Delta x$, &c. Hence follows, according to (20.), the most probable value of X,

$$\left.\begin{array}{l} X - V = 0, \\ \text{and the probable error of } X - V, \\ F = \sqrt{\left\{\left(\frac{d V}{d a}\right)^2 r^2 + \left(\frac{d V}{d a'}\right)^2 r'^2 + \left(\frac{d V}{d a''}\right)^2 r''^2 + \ldots\right\}} \end{array}\right\} \quad (23.)$$

or the most probable value of X is V, and the probable error of this determination is equal to the above-determined F; a solution which is rigorously true for linear functions, but only approximately so for higher ones.

It is a different case, supposing that we have found for *one* and the *same* unknown x, by different examinations, the values $a, a', a'' \ldots$, with the probable errors $r, r', r'' \ldots$, or the weights $p, p', p'' \ldots$, and that we seek to find from them all the most probable values. The definition of the idea of weight, according to which a, a', a'' must be considered as respectively found by the number p, p', p'', of equally good observations, gives here, by virtue of the arithmetical mean, the most probable value of x

$$x = \frac{ap + a'p' + a''p'' +, \&c.}{p + p' + p'' +, \&c.},$$

with the weight

$$p + p' + p'' +, \&c.;$$

or, which is the same thing, the most probable value of

$$\left.\begin{array}{l} x = \dfrac{\frac{a}{r^2} + \frac{a'}{r'^2} + \frac{a'}{r''^2} +, \&c.}{\frac{1}{r^2} + \frac{1}{r'^2} + \frac{1}{r''^2} +, \&c.} \\ \text{with the probable error} \\ = \dfrac{1}{\sqrt{\left(\frac{1}{r^2} + \frac{1}{r'^2} + \frac{1}{r''^2} +, \&c.\right)}} \end{array}\right\} \quad . \quad . \quad (24.)$$

$$\int_0^t \frac{2\,e^{-t^2}\,dt}{\sqrt{\pi}} = \Theta(t)$$

t	$\Theta(t)$			t	$\Theta(t)$		
0·00	0·00000 00	1128 33	0	0·50	0·52049 99	874 38	8 78
0·01	01128 33	1128 11	22	0·51	52924 37	865 50	8 88
0·02	02256 44	1127 66	45	0·52	53789 87	856 54	8 96
0·03	03384 10	1126 99	67	0·53	54646 41	847 51	9 03
0·04	04511 09	1126 09	90	0·54	55493 92	838 41	9 10
0·05	05637 18	1124 97	1 12	0·55	56332 33	829 24	9 17
0·06	06762 15	1123 62	1 35	0·56	57161 57	820 01	9 23
0·07	07885 77	1122 04	1 58	0·57	57981 58	810 71	9 30
0·08	09007 81	1120 25	1 79	0·58	58792 29	801 36	9 35
0·09	10128 06	1118 24	2 01	0·59	59593 65	791 96	9 40
0·10	0·11246 30	1116 00	2 24	0·60	0·60385 61	782 51	9 45
0·11	12362 30	1113 54	2 46	0·61	61168 12	773 02	9 49
0·12	13475 84	1110 87	2 67	0·62	61941 14	763 49	9 53
0·13	14586 71	1107 99	2 88	0·63	62704 63	753 94	9 55
0·14	15694 70	1104 89	3 10	0·64	63458 57	744 35	9 59
0·15	16799 59	1101 58	3 31	0·65	64202 92	734 73	9 62
0·16	17901 17	1098 06	3 52	0·66	64937 65	725 10	9 63
0·17	18999 23	1094 34	3 72	0·67	65662 75	715 45	9 65
0·18	20093 57	1090 41	3 93	0·68	66378 20	705 79	9 66
0·19	21183 98	1086 27	4 14	0·69	67083 99	696 11	9 68
0·20	0·22270 25	1081 93	4 34	0·70	0·67780 10	686 44	9 67
0·21	23352 18	1077 40	4 53	0·71	68466 54	676 76	9 68
0·22	24429 58	1072 67	4 73	0·72	69143 30	667 08	9 68
0·23	25502 25	1067 75	4 92	0·73	69810 38	657 42	9 66
0·24	26570 00	1062 63	5 12	0·74	70467 80	647 76	9 66
0·25	27632 63	1057 34	5 29	0·75	71115 56	638 11	9 65
0·26	28689 97	1051 85	5 49	0·76	71753 67	628 49	9 62
0·27	29741 82	1046 18	5 67	0·77	72382 16	618 88	9 61
0·28	30788 00	1040 34	5 84	0·78	73001 04	609 31	9 57
0·29	31828 34	1034 33	6 01	0·79	73610 35	599 75	9 56
0·30	0·32862 67	1028 14	6 19	0·80	0·74210 10	590 23	9 52
0·31	33890 81	1021 78	6 36	0·81	74800 33	580 75	9 48
0·32	34912 59	1015 26	6 52	0·82	75381 08	571 30	9 45
0·33	35927 85	1008 59	6 67	0·83	75952 38	561 89	9 41
0·34	36936 44	1001 75	6 84	0·84	76514 27	552 53	9 36
0·35	37938 19	994 77	6 98	0·85	77066 80	543 22	9 31
0·36	38932 96	987 63	7 14	0·86	77610 02	533 96	9 26
0·37	39920 59	980 34	7 29	0·87	78143 98	524 75	9 21
0·38	40900 93	972 92	7 42	0·88	78668 73	515 59	9 16
0·39	41873 85	965 37	7 55	0·89	79184 32	506 50	9 09
0·40	0·42839 22	957 68	7 69	0·90	0·79690 82	497 46	9 04
0·41	43796 90	949 86	7 82	0·91	80188 28	488 49	8 97
0·42	44746 76	941 91	7 95	0·92	80676 77	479 58	8 91
0·43	45688 67	933 84	8 07	0·93	81156 35	470 75	8 83
0·44	46622 51	925 67	8 17	0·94	81627 10	461 98	8 77
0·45	47548 18	917 37	8 30	0·95	82089 08	453 28	8 70
0·46	48465 55	908 97	8 40	0·96	82542 36	444 67	8 61
0·47	49374 52	900 46	8 53	0·97	82987 03	436 12	8 55
0·48	50274 98	891 85	8 61	0·98	83423 15	427 66	8 46
0·49	51166 83	883 16	8 69	0·99	83850 81	419 27	8 39
0·50	0·52049 99		8 78	1·00	0·84270 08		8 30

$$\int_0^t \frac{2\,e^{-t^2}\,dt}{\sqrt{\pi}} = \Theta\,(t)$$

t	$\Theta\,(t)$			t	$\Theta\,(t)$		
1·00	0·84270 08	410 97	8 30	1·50	0·96610 52	117 16	3 57
1·01	84681 05	402 75	8 22	1·51	96727 68	113 67	3 49
1·02	85083 80	394 62	8 13	1·52	96841 35	110 27	3 40
1·03	85478 42	386 57	8 05	1·53	96951 62	106 95	3 32
1·04	85864 99	378 61	7 96	1·54	97058 57	103 70	3 25
1·05	86243 60	370 75	7 86	1·55	97162 27	100 54	3 16
1·06	86614 35	362 97	7 77	1·56	97262 81	97 45	3 09
1·07	86977 32	355 29	7 68	1·57	97360 26	94 44	3 01
1·08	87332 61	347 69	7 60	1·58	97454 70	91 50	2 94
1·09	87680 30	340 20	7 49	1·59	97546 20	88 64	2 86
1·10	0·88020 50	332 80	7 40	1·60	0·97634 84	85 85	2 79
1·11	88353 30	325 49	7 31	1·61	97720 69	83 12	2 73
1·12	88678 79	318 28	7 21	1·62	97803 81	80 48	2 64
1·13	88997 07	311 16	7 12	1·63	97884 29	77 89	2 59
1·14	89308 23	304 15	7 01	1·64	97962 18	75 38	2 51
1·15	89612 38	297 24	6 91	1·65	98037 56	72 93	2 45
1·16	89909 62	290 42	6 82	1·66	98110 49	70 55	2 38
1·17	90200 04	283 70	6 72	1·67	98181 04	68 24	2 31
1·18	90483 74	277 09	6 61	1·68	98249 28	65 98	2 26
1·19	90760 83	270 57	6 52	1·69	98315 26	63 78	2 20
1·20	0·91031 40	264 15	6 42	1·70	0·98379 04	61 66	2 12
1·21	91295 55	257 84	6 31	1·71	98440 70	59 58	2 08
1·22	91553 39	251 62	6 22	1·72	98500 28	57 57	2 01
1·23	91805 01	245 51	6 11	1·73	98557 85	55 61	1 96
1·24	92050 52	239 49	6 02	1·74	98613 46	53 71	1 90
1·25	92290 01	233 58	5 91	1·75	98667 17	51 86	1 85
1·26	92523 59	227 77	5 81	1·76	98719 03	50 07	1 79
1·27	92751 36	222 06	5 71	1·77	98769 10	48 32	1 75
1·28	92973 42	216 45	5 61	1·78	98817 42	46 64	1 68
1·29	93189 87	210 93	5 52	1·79	98864 06	44 99	1 65
1·30	0·93400 80	205 52	5 41	1·80	0·98909 05	43 40	1 59
1·31	93606 32	200 20	5 32	1·81	98952 45	41 86	1 54
1·32	93806 52	194 98	5 22	1·82	98994 31	40 36	1 50
1·33	94001 50	189 87	5 11	1·83	99034 67	38 92	1 44
1·34	94191 37	184 85	5 02	1·84	99073 59	37 51	1 41
1·35	94376 22	179 92	4 93	1·85	99111 10	36 15	1 36
1·36	94556 14	175 10	4 82	1·86	99147 25	34 82	1 33
1·37	94731 24	170 36	4 74	1·87	99182 07	33 55	1 27
1·38	94901 60	165 73	4 63	1·88	99215 62	32 31	1 24
1·39	95067 33	161 18	4 55	1·89	99247 93	31 11	1 20
1·40	0·95228 51	156 73	4 45	1·90	0·99279 04	29 95	1 16
1·41	95385 24	152 38	4 35	1·91	99308 99	28 83	1 12
1·42	95537 62	148 11	4 27	1·92	99337 82	27 75	1 08
1·43	95685 73	143 93	4 18	1·93	99365 57	26 69	1 06
1·44	95829 66	139 84	4 09	1·94	99392 26	25 68	1 01
1·45	95969 50	135 85	3 99	1·95	99417 94	24 69	99
1·46	96105 35	131 94	3 91	1·96	99442 63	23 74	95
1·47	96237 29	128 12	3 82	1·97	99466 37	22 83	91
1·48	96365 41	124 38	3 74	1·98	99489 20	21 94	89
1·49	96489 79	120 73	3 65	1·99	99511 14	21 09	85
1·50	0·96610 52		3 57	2·00	0·99532 23		82

$$\int_0^{\varrho \frac{\Delta}{r}} \frac{2}{\sqrt{\pi}} e^{-t^2}\, dt = \Theta\left(\varrho \frac{\Delta}{r}\right) \qquad \varrho = 0{\cdot}4769360$$

$\frac{\Delta}{r}$	$\Theta\left(\varrho\frac{\Delta}{r}\right)$		$\frac{\Delta}{r}$	$\Theta\left(\varrho\frac{\Delta}{r}\right)$		$\frac{\Delta}{r}$	$\Theta\left(\varrho\frac{\Delta}{r}\right)$	
0·00	0·00000	538	0·40	0·21268	519	0·80	0·41052	465
0·01	00538	538	0·41	21787	517	0·81	41517	462
0·02	01076	538	0·42	22304	517	0·82	41979	461
0·03	01614	538	0·43	22821	515	0·83	42440	459
0·04	02152	538	0·44	23336	515	0·84	42899	458
0·05	02690	538	0·45	23851	513	0·85	43357	456
0·06	03228	538	0·46	24364	512	0·86	43813	454
0·07	03766	537	0·47	24876	512	0·87	44267	452
0·08	04303	537	0·48	25388	510	0·88	44719	450
0·09	04840	538	0·49	25898	509	0·89	45169	449
0·10	0·05378	536	0·50	0·26407	508	0·90	0·45618	446
0·11	05914	537	0·51	26915	506	0·91	46064	445
0·12	06451	536	0·52	27421	506	0·92	46509	443
0·13	06987	536	0·53	27927	504	0·93	46952	441
0·14	07523	536	0·54	28431	503	0·94	47393	439
0·15	08059	535	0·55	28934	502	0·95	47832	438
0·16	08594	535	0·56	29436	500	0·96	48270	435
0·17	09129	534	0·57	29936	499	0·97	48605	434
0·18	09663	534	0·58	30435	498	0·98	49139	431
0·19	10197	534	0·59	30933	497	0·99	49570	430
0·20	0·10731	533	0·60	0·31430	495	1·00	0·50000	428
0·21	11264	532	0·61	31925	494	1·01	50428	425
0·22	11796	532	0·62	32419	492	1·02	50853	424
0·23	12328	532	0·63	32911	491	1·03	51277	422
0·24	12860	531	0·64	33402	490	1·04	51699	420
0·25	13391	530	0·65	33892	488	1·05	52119	418
0·26	13921	530	0·66	34380	486	1·06	52537	415
0·27	14451	529	0·67	34866	486	1·07	52952	414
0·28	14980	528	0·68	35352	483	1·08	53366	412
0·29	15508	527	0·69	35835	482	1·09	53778	410
0·30	0·16035	527	0·70	0·36317	481	1·10	0·54188	407
0·31	16562	526	0·71	36798	479	1·11	54595	406
0·32	17088	526	0·72	37277	478	1·12	55001	403
0·33	17614	524	0·73	37755	476	1·13	55404	402
0·34	18138	524	0·74	38231	474	1·14	55806	399
0·35	18662	523	0·75	38705	473	1·15	56205	397
0·36	19185	522	0·76	39178	471	1·16	56602	396
0·37	19707	522	0·77	39649	469	1·17	56998	393
0·38	20229	520	0·78	40118	468	1·18	57391	391
0·39	20749	519	0·79	40586	466	1·19	57782	389
0·40	0·21268		0·80	0·41052		1·20	0·58171	

$$\int_0^{\varrho\frac{\Delta}{r}} \frac{2}{\sqrt{\pi}}\, e^{-t^2}\, dt = \Theta\left(\varrho\, \frac{\Delta}{r}\right) \qquad \varrho = 0{\cdot}4769360$$

$\frac{\Delta}{r}$	$\Theta\left(\varrho\frac{\Delta}{r}\right)$		$\frac{\Delta}{r}$	$\Theta\left(\varrho\frac{\Delta}{r}\right)$		$\frac{\Delta}{r}$	$\Theta\left(\varrho\frac{\Delta}{r}\right)$	
1·20	0·58171	387	1·60	0·71949	300	2·00	0·82266	215
1·21	58558	384	1·61	72249	297	2·01	82481	214
1·22	58942	383	1·62	72546	295	2·02	82695	212
1·23	59325	380	1·63	72841	293	2·03	82907	210
1·24	59705	378	1·64	73134	291	2·04	83117	207
1·25	60083	377	1·65	73425	289	2·05	83324	206
1·26	60460	373	1·66	73714	286	2·05	83530	204
1·27	60833	372	1·67	74000	285	2·07	83734	202
1·28	61205	370	1·68	74285	282	2·08	83936	201
1·29	61575	367	1·69	74567	280	2·09	84137	198
1·30	0·61942	366	1·70	0·74847	277	2·10	0·84335	196
1·31	62308	363	1·71	75124	276	2·11	84531	195
1·32	62671	361	1·72	75400	274	2·12	84726	193
1·33	63032	359	1·73	75674	271	2·13	84919	190
1·34	63391	356	1·74	75945	269	2·14	85109	189
1·35	63747	355	1·75	76214	267	2·15	85298	188
1·36	64102	352	1·76	76481	265	2·16	85486	185
1·37	64454	350	1·77	76746	263	2·17	85671	183
1·38	64804	348	1·78	77009	261	2·18	85854	182
1·39	65152	346	1·79	77270	258	2·19	86036	180
1·40	0·65498	343	1·80	0·77528	257	2·20	0·86216	178
1·41	65841	341	1·81	77785	254	2·21	86394	176
1·42	66182	339	1·82	78039	252	2·22	86570	175
1·43	66521	337	1·83	78291	251	2·23	86745	172
1·44	66858	335	1·84	78542	248	2·24	86917	171
1·45	67193	333	1·85	78790	246	2·25	87088	170
1·46	67526	330	1·86	79036	244	2·26	87258	167
1·47	67856	328	1·87	79280	242	2·27	87425	166
1·48	68184	326	1·88	79522	239	2·28	87591	164
1·49	68510	323	1·89	79761	238	2·29	87755	163
1·50	0·68833	322	1·90	0·79999	236	2·30	0·87918	160
1·51	69155	319	1·91	80235	234	2·31	88078	159
1·52	69474	317	1·92	80469	231	2·32	88237	158
1·53	69791	315	1·93	80700	230	2·33	88395	155
1·54	70106	313	1·94	80930	228	2·34	88550	155
1·55	70419	310	1·95	81158	225	2·35	88705	152
1·56	70729	309	1·96	81383	224	2·36	88857	151
1·57	71038	306	1·97	81607	221	2·37	89008	149
1·58	71344	304	1·98	81828	220	2·38	89157	147
1·59	71648	301	1·99	82048	218	2·39	89304	146
1·60	0·71949		2·00	0·82266		2·40	0·89450	

$$\int_0^{\varrho\frac{\Delta}{r}} \frac{2}{\sqrt{\pi}} e^{-t^2}\, dt = \Theta\left(\varrho \frac{\Delta}{r}\right) \qquad \varrho = 0{\cdot}4769360$$

$\frac{\Delta}{r}$	$\Theta\left(\varrho\frac{\Delta}{r}\right)$		$\frac{\Delta}{r}$	$\Theta\left(\varrho\frac{\Delta}{r}\right)$		$\frac{\Delta}{r}$	$\Theta\left(\varrho\frac{\Delta}{r}\right)$	
2·40	0·89450	145	2·80	0·94105	90	3·20	0·96910	52
2·41	89595	143	2·81	94195	89	3·21	96962	51
2·42	89738	141	2·82	94284	87	3·22	97013	51
2·43	89879	140	2·83	94371	87	3·23	97064	50
2·44	90019	138	2·84	94458	85	3·24	97114	49
2·45	90157	136	2·85	94543	84	3·25	97163	48
2·46	90293	135	2·86	94627	84	3·26	97211	48
2·47	90428	134	2·87	94711	82	3·27	97259	47
2·48	90562	132	2·88	94793	81	3·28	97306	46
2·49	90694	131	2·89	94874	80	3·29	97352	45
2·50	0·90825	129	2·90	0·94954	79	3·30	0·97397	45
2·51	90954	128	2·91	95033	78	3·31	97442	44
2·52	91082	126	2·92	95111	76	3·32	97486	44
2·53	91208	124	2·93	95187	76	3·33	97530	43
2·54	91332	124	2·94	95263	75	3·34	97573	42
2·55	91456	122	2·95	95338	74	3·35	97615	42
2·56	91578	120	2·96	95412	73	3·36	97657	41
2·57	91698	119	2·97	95485	72	3·37	97698	40
2·58	91817	118	2·98	95557	71	3·38	97738	40
2·59	91935	116	2·99	95628	70	3·39	97778	39
2·60	0·92051	115	3·00	0·95698	69	3·40	0·97817	359
2·61	92166	114	3·01	95767	68	3·50	98176	306
2·62	92280	112	3·02	95835	67	3·60	98482	261
2·63	92392	111	3·03	95902	66	3·70	98743	219
2·64	92503	110	3·04	95968	65	3·80	98962	185
2·65	92613	108	3·05	96033	65	3·90	99147	155
2·66	92721	107	3·06	96098	63	4·00	99302	129
2·67	92828	106	3·07	96161	63	4·10	99431	108
2·68	92934	104	3·08	96224	62	4·20	99539	88
2·69	93038	103	3·09	96286	60	4·30	99627	73
2·70	0·93141	102	3·10	0·96346	60	4·40	0·99700	60
2·71	93243	101	3·11	96406	60	4·50	99760	48
2·72	93344	99	3·12	96466	58	4·60	99808	40
2·73	93443	98	3·13	96524	58	4·70	99848	31
2·74	93541	97	3·14	96582	56	4·80	99879	26
2·75	93638	96	3·15	96638	56	4·90	99905	21
2·76	93734	94	3·16	96694	55	5·00	99926	
2·77	93828	94	3·17	96749	55			
2·78	93922	92	3·18	96804	53			
2·79	94014	91	3·19	96857	53			
2·80	0·94105		3·20	0·96910				

Article XI.

On the Theory of the Formation of Æther. By Heinrich Rose, Professor of Chemistry in the University of Berlin.*

[From Poggendorff's *Annalen*, vol. xlviii., part 11, November 1839.]

It is well known that many salts of the oxide of bismuth, of the oxide of mercury, of antimony, and several other metallic oxides are decomposed by water. They are generally converted by it into basic salts; but sometimes, by employing a sufficient quantity of water, the decomposition even goes to the separation of the pure oxide, as in the case of the nitrate of the oxide of mercury.

The explanation usually given of these decompositions is, that the water resolves the neutral salt of a metallic oxide into an acid and a basic salt, in a similar manner as nitric acid converts the red superoxide of lead into protoxide of lead and the brown superoxide. But the existence of acid salts, which are said to be formed by the action of water on several neutral salts of metallic oxides, is far from being proved; in most cases the water only deprives the salt of a part of the acid, and this dissolves a portion of the neutral salt, which, after the acid solution has been concentrated by evaporation, most frequently crystallizes as a neutral salt, and rarely as a double combination of neutral salt and acid hydrate. In many cases the quantity of the salt which dissolves in the liberated acid is exceedingly small, frequently none at all, and the entire quantity of the oxide forms an insoluble basic salt.

The simplest explanation that can be given of such decompositions produced by water, appears to me to be this, that water, acting the part of a base, separates the metallic oxide as a basic salt, or at times even in the pure state, and combines with the acid to form a hydrate. This explanation is the more admissible, as we have been long accustomed to regard the hydrates of acids as saline combinations in which water replaces a fixed base. It is well known what happy conclusions for the whole theory of chemistry, more espe-

* Translated and communicated by Mr. William Francis.

cially Graham, Berzelius and Liebig, have drawn from this view.

In fact it is particularly the salts of such metallic oxides as are not possessed of strong basic properties that are decomposed by water. The salts of the powerful bases do not exhibit this phænomenon.

According to this view the decompositions in question are analogous to the conversion of the red oxide of lead into the brown superoxide and protoxide of lead, by nitric acid, only that they are of exactly the converse kind, the strong acid expelling from a combination of the oxide of lead, with oxygen, the weaker electro-negative body, and combining with the basic.

Water also occurs in other cases as a base, and sometimes displaces other bases from their combinations. As it, however, belongs to the weaker bases, and, at the same time, is volatile, these cases are not very frequent; but although volatile, it is nevertheless able to expel the more volatile oxide of ammonium from its combinations. If a solution of the sulphate of the oxide of ammonium be boiled for a long time, it becomes acid; and if the boiling is effected in a retort, a liquid passes over into the recipient, which contains free ammonia. This result evidently arises from the water, as base, eliminating the oxide of ammonium (which cannot exist in a free state, and passes into water and ammonia) from its combination with the sulphuric acid, and combining with the same. The quantity of the sulphate of the oxide of ammonium decomposed in this way, is indeed but small; but it must also be remembered that the oxide of ammonium is one of the most powerful bases, and this result is chiefly to be ascribed to its greater volatility.

If we apply the above explanation of the decomposition of many salts by water to the theory of the formation of æther, it will acquire great simplicity.

Berzelius and Liebig have advanced the view that æther may be regarded as a base; which has found such general assent, that it is almost universally adopted, at least in Germany.

It is well known that the salts of the oxide of æthyl (the compound æthers) may be more or less easily decomposed by bases, water being present; the bases combine with the acid of the compound, and separate the oxide of æthyl as a hydrate (alcohol).

But water itself, which in this case acts evidently the part of

a base, also causes the same decomposition. Some compounds of the oxide of æthyl are as easily decomposed by water as by many bases; so, for instance, is oxalic æther, which is converted by water into hydrate of oxalic acid and alcohol. A high temperature is not even requisite to produce this change; for it takes place at the common temperature, and indeed in a very short time.

But the acid sulphate of the oxide of æthyl,—or, rather the combination of the sulphate of the oxide of æthyl, with hydrated sulphuric acid (sulphovinic acid), also undergoes in its solution in water quite a similar decomposition. Even at the usual temperature alcohol and the hydrate of sulphuric acid are gradually formed; it proceeds more rapidly by boiling. This process may likewise be most easily explained by the supposition that water acting as a base eliminates the oxide of æthyl from its combination with sulphuric acid, which, at the moment of its expulsion, takes up water and forms alcohol.

The solutions of nearly all sulphovinates in water are decomposed, especially on boiling, in a similar manner. Alcohol and water evaporate, and a so-called acid sulphate, *i. e.* a double compound of the neutral salt, which already pre-existed in the sulphovinate salt with the hydrate of sulphuric acid, is formed in the solution.

If sulphovinic acid is heated with merely a small quantity of water, no alcohol is obtained, but chiefly hydrated sulphuric acid, and pure oxide of æthyl or æther:—there is not sufficient water present to convert the liberated æther into alcohol.

If alcohol is mixed with the hydrate of sulphuric acid, sulphovinic acid is formed, or a double compound of the neutral sulphate of the oxide of æthyl with the hydrated sulphuric acid. By the formation of the sulphate of the oxide of æthyl two atoms of water are set free, one from the hydrated sulphuric acid, the other from the alcohol. On heating the mixture, one of these liberated atoms of water eliminates oxide of æthyl from its combination with sulphuric acid, and combines with the acid, forming the hydrate of sulphuric acid.

But why does not the æther at the moment of its expulsion combine with water and form alcohol? There is sufficient water present, for only one atom of water is requisite to expel the æther; and at the formation of sulphovinic acid, even when anhydrous alcohol is employed, two atoms are set free.

It is well known that sulphuric acid can take up more than one atom of water to form a hydrate. Besides the common hydrate, with one atom of water, we are acquainted with a second, which may be prepared in a crystalline state, and contains two atoms of water. This combination corresponds to a basic sulphate salt.

The disposition of the hydrate of sulphuric acid to take up more water is very great, and it is employed on this account for various purposes in our laboratories. It is this which prevents the æther, originating from the decomposition of the sulphovinic acid, from taking up the second atom of water; but if the mixture is uninterruptedly boiled for some time, the hydrated sulphuric acid loses the acquired water, which may then be distilled over in company with the æther. The æther therefore may be distilled over at the same time with water, from a boiling mixture of the hydrate of sulphuric acid and alcohol; but they are not the products of one, but of two chemical processes, which are both active together in the boiling mixture.

At the commencement of the operation but very little water passes over along with the æther and that alcohol contained in the mixture, which has not been converted into sulphovinic acid, so that the water remains dissolved in the distilled alcoholic æther, and does not separate: the quantity of water increases by further distillation, especially at a high temperature, when the quantity of the second hydrate of sulphuric acid has augmented.

Alcohol is scarcely ever employed anhydrous in the preparation of æther, but generally hydrated. It is evident that in the latter case the quantity of the second hydrate of sulphuric acid must be considerably increased. The experiments of Liebig, Magnus, and Marchand have shown that in the cold this second hydrate cannot form sulphovinic acid with alcohol, but does so at a higher temperature, and therefore that such a mixture on boiling can give æther by distillation. But it is well known that on employing hydrated, or even anhydrous alcohol, there is always a portion of it which is not converted into sulphovinic acid, and this quantity may be distilled as alcohol from the mixture. A second portion of alcohol, which distils over in company with the æther, in the formation of æther, may, however, be produced in this way,—that æther and water are cotemporaneously disengaged from the mixture, and com-

bine to form alcohol; for it is produced only in this way when a solution of pure sulphovinic acid is boiled with much water, or compound æthers decomposed by water or by the hydrates of bases.

When, however, from the tendency of the hydrate of sulphuric acid to take up more water, æther has been evolved from a mixture of alcohol and sulphuric acid, it does not take up any water after being once separated: but water may be distilled over by heating the diluted sulphuric acid. We know that when æther is treated with water, or even dissolved in it, no alcohol is formed. When æther is once separated from a compound of oxide of æthyl, the former can in no way be converted by water into alcohol. Only when, as above observed, the æther comes in contact with water at the moment of its expulsion does it form alcohol with it. The cotemporaneous disengagement of æther and water, from a boiling mixture of alcohol and the hydrate of sulphuric acid, shows therefore quite evidently that both owe their origin to two distinct processes.

Moreover, it is by no means an anomalous phænomenon that a base, which is capable of forming a hydrate, does not combine with water when brought into contact with it in a pure state; a great number of cases of this kind occur in inorganic chemistry. We need only compare æther with that numerous class of ignited oxides in which so compact a state of cohesion is produced by heat, that they not only withstand the action of water, but even entirely or partially that of acids, to find abundant proof of such analogies. The ignited oxides with these properties always belong to the weaker bases, under which æther must incontrovertibly be classed. Æther may be assimilated to these oxides the more, as it like them combines directly with acids with difficulty.

But even among the stronger bases we find some whose relations to water resemble those of æther. When oxide of copper is precipitated in the cold by bases from solutions of salts of the oxides of copper, it appears as hydrate of the oxide of copper; which, however, on being heated under water, loses its water, and does not take it up again when left in contact with it at a higher, or at the common temperature.

To find out at what period, in the preparation of æther by boiling a mixture of alcohol and sulphuric acid, water commences to pass over, M. Wittstock, at my request, instituted a

series of experiments, which he had the kindness to communicate to me.

Two pounds of the hydrate of sulphuric acid were mixed cold with two pounds of anhydrous alcohol, the mixture was made to boil with all possible haste in a retort, the distilled products, well cooled, were gradually received, and the distillation continued until the contents of the retort boiled over.

The weight and specific gravity of the products were determined as they distilled over in succession. The results are as follow:

First product: 3 drachms 50 grains; spec. gr. 0·776*; produced before the boiling of the mixture. The following products passed over after its boiling:

Second product: 3 ounces 6 drachms; spec. gr. 0·808.

Third product: 3 ounces 6 drs.; spec. gr. 0·800.

Fourth product: 3 ounces 6 drs.; spec. gr. 0·786.

Fifth product: 5 ounces 3 drs. 50 grs.; spec. gr. 0·776.

Sixth product: 4 ounces 1 dr. 50 grs.; spec. gr. 0761.

Seventh product: 1 ounce 7 drs. 10 grs.; spec. gr. 0·809.

Eighth product: 1 ounce 2 drs.

The first five products consisted of a single liquid; the sixth was the first in which a layer of water and of æther were perceptible. The quantity of separated water amounted to 3 drachms; the æthereal liquid had the specific gravity mentioned above. The seventh product consisted in volume of two parts water, and three parts of an æthereal fluid of the specific gravity stated; the eighth consisted almost entirely of water, above which floated a very thin layer of æther, which was coloured yellow by oil of wine. The contents of the retort boiled over on the continued application of heat.

The first five products consisted of æther mixed with alcohol, which last was contained in the retort as such, and not converted into sulphovinic acid, and evaporated from the mixture in company with the æther. The first product, which distilled over at the lowest temperature, contained, to judge from its specific gravity, much æther, and little alcohol, quite opposed to the general opinion that æther is only formed at the boiling-point of the mixture. The succeeding products gradually became, according to their specific gravity, constantly more æthereal, and

* The specific gravities, both here as well as those to be mentioned subsequently, were all determined at 14° Reaum. (63·5° Fahr.).

contained less alcohol; but only in the sixth product was there so much water that it separated, and the quantity increased in proportion as the distillation was continued.

The first six products smelt but slightly of oil of wine; but the seventh contained a portion, and also smelt of sulphurous acid. After the first seven products had been mixed together, and the separated water removed, they had a specific gravity of 0·788.

It is well known that æther is prepared, of late, in the most advantageous manner, by allowing a small stream of alcohol to flow constantly into a mixture of alcohol and the hydrate of sulphuric acid, and distilling off æther in proportion as alcohol is added*. It has been denied that the presence of sulphovinic acid is of essential influence in the formation of æther, and asserted that it is not necessary that the formation of this acid should precede that of æther, because in the method of preparing æther alluded to, the boiling mixture must be constantly at a temperature of 140° cent., at which sulphovinic acid could not exist. But at the point where the current of cold alcohol flows into the boiling mixture, the temperature is under 140°. The sulphovinic acid formed is decomposed it is true, in a very short time, from its soon acquiring the temperature of the boiling liquid. The preparation of æther, according to the above method, consists therefore in a constant formation, and continual decomposition of sulphovinic acid. It is a pretty generally entertained opinion that the production of æther from a mixture of alcohol and sulphuric acid, is solely effected by the boiling of the mixture, which takes place at a high temperature, about 140° cent. In many works on chemistry we meet with the assertion that when a mixture of sulphuric acid and alcohol are heated at a temperature, not high enough for it to boil, no æther, but merely anhydrous alcohol, is obtained.

Were this assertion correct, it would be an important objection to the hypothesis I have advanced; for, according to that, it would be somewhat difficult to explain the circumstance why the oxide of æthyl is separated at a lower temperature, as a hydrate, and at a higher one in an anhydrous state.

But this common opinion is founded on an error, which to me is quite incomprehensible. Æther is obtained even from a mixture of the hydrate of sulphuric acid and anhydrous alcohol,

* See Poggendorff's *Annalen*, vol. xx. p. 461.

when distilled in a water-bath, at a temperature which need not always amount to the boiling heat of water. It is not indeed requisite to employ anhydrous alcohol, but the hydrated, of 90 per cent. Tralles*, to obtain æther from a mixture at the above-mentioned temperature.

M. Wittstock had the goodness, at my request, to institute a series of experiments on this point, and communicated the results to me.

I. Fifteen ounces of anhydrous alcohol were mixed in the cold, with an equal weight of the hydrate of sulphuric acid, and the mixture distilled at a temperature at which it could not boil strongly. The products, well cooled, were successively received, and the temperature at which they passed over accurately observed.

First product: 1 dr. 10 grs., spec. gr. 0·817, passed over at from	60° to 80° R.
Second product: 3 oz. 1 dr. 10 gr., spec. gr. 0·792, passed over at from	90° „ 93° „
Third product: 3 drs. 57 grs., spec. gr. 0·772, passed over at from	75° „ 80° „
Fourth product: 2 oz. 40 grs., spec. gr. 0·749, passed over at from	90° „ 95° „
Fifth product: 5 drs.	

When the mixture had reached the temperature of 90° it began to boil very slightly; the boiling, however, subsequently ceased at this temperature, but even then æther was disengaged from the mixture in bubbles, just as carbonic acid gas escapes at the common temperature from a liquid strongly saturated with it.

From these experiments it is evident that æther is formed at far lower temperatures than is usually supposed. The first product smelt indeed strongly of æther; but chiefly consisted, which is also indicated by the specific gravity, of alcohol, which had not been converted, by mixing with sulphuric acid, into sulphovinic acid; æther could not be separated from it, either by water or even by chloride of calcium. The second, third, and fourth products consisted, on the contrary, principally of æther, which could even be separated by mere washing with

* That is, 90 per cent. absolute alcohol by volume; when in Germany it is reckoned by weight, Richter's scale is employed. In Prussia alcoholometers after Tralles are employed by the Excise.

water. The fifth was the first that contained free water, and indeed, in volume, more than the half. The specific gravity of the æthereal liquid floating above it was not determined. This last product distilled over very slowly, although at times the temperature was raised to 100° R.

It results from these experiments that æther which is produced at lower temperatures than is requisite to boil the mixture, is at the same time purer, and contains less alcohol and water than æther which has been prepared by strong boiling. A comparison of the specific gravities with those previously mentioned, set this evidently beyond all doubt. At a low temperature the water especially escapes later, and therefore only in the last product could separated water be observed, a proof that it is not disengaged in company with the æther.

II. A second series of experiments proved this in a still more decided manner, so that there can no longer remain any doubt on the subject that æther can be evolved in abundant quantity at the boiling-point of water.

Seventeen ounces of anhydrous alcohol of specific gravity 0·792 were mixed cold with 18 ounces of the hydrate of sulphuric acid, and the mixture subjected to distillation in a water-bath whose temperature frequently did not even attain that of boiling water. The quantities taken are in the proportion of single equivalents of each of the substances employed; they were taken in this proportion, partly because it approaches that which otherwise is employed in the preparation of æther, when equal parts by weight of alcohol and sulphuric acid are employed, and also in order to have no excess of sulphuric acid.

The results of the experiments are as follow:

First product: 3 drs.

Second product: 3 oz. 6 drs.; spec. gr. 0·755.

Third product: 3 drs.; spec. gr. 0·745.

Fourth product.

Even the first product consisted of nearly pure æther; for a solution of acetate of potash separated æther from the liquid to the amount of two thirds of its volume.

The fourth and last products contained free water, and consisted of nearly half of it by volume; but it distilled over so slowly in the water-bath, that several hours were necessary to obtain a few drachms of it. From the specific gravities it will

be perceived that the second, and especially the third product, consisted of æther far more pure than is obtained in other modes of preparing that substance.

III. As the idea is so general, that æther is formed from a mixture of alcohol and sulphuric acid only on boiling, and as in the usual mode of distilling, hydrated, and not anhydrous alcohol is employed, a new series of experiments were performed with the former.

A pound of alcohol of 90° Tralles, such as is usually employed in the preparation of æther, was mixed in the cold with a pound of the hydrate of sulphuric acid, and the mixture subjected to distillation in a water-bath, as in the second series of experiments. The results were:

First product: 4 drs. 36 grs.; spec. gr. 0·833.
Second product: 2 oz. 4 drs. 20 grs.; spec. gr. 0·787.
Third product: 4 drs. 50 grs.; spec. gr. 0·789.
Fourth product: 5 drs. 17 grs.; spec. gr. 0·789.
Fifth product.

The first product consisted almost entirely of alcohol, as indicated by the specific gravity. The succeeding ones contained much æther, or consisted mostly of it. Free water also was evident in this case only in the fifth and last product, which consisted of 1 drachm of liquid, of which only one fourth was separated water. To distil this small quantity over, it was necessary to heat for more than five hours.

The æther obtained from a mixture of sulphuric acid and alcohol, at the temperature of boiling water, is far more pure, as may be anticipated, and is indicated by the specific gravities of the products, when anhydrous, instead of hydrated, alcohol is employed. The æther obtained from hydrated alcohol in this way contains more alcohol, because upon mixing hydrated alcohol with sulphuric acid, less is converted into sulphovinic acid, and more remains in a free state in the mixture, than when absolute alcohol is used. According, however, to the theory advanced in this memoir, only that portion of the coalhol can produce æther which has been converted into sulphovinic acid, and this æther distils over when heated, in company with the free alcohol.

The fact that æther is produced from a mixture of alcohol and sulphuric acid even at the boiling-point of water, is indeed highly important in the theory of the formation of æther, and

by this method the æther is also obtained more pure, especially from water, and of a far lower specific gravity than when distilled at a boiling heat; but it is not convenient in the preparation of æther, in so far as at this low temperature the æther, and particularly the last products, pass over with great slowness.

One fact, however, seems not to admit of being quite satisfactorily explained by the present theory. Seeing that water acts as a base upon the oxide of æthyl, and disengages it from its combinations, it must appear surprising that stronger bases than water do not effect this separation still more perfectly. But solutions of the sulphovinate of potash and soda may be treated with an excess of potash without the oxide of æthyl being expelled; and even the salts of the alkaline earths can exist in contact with an excess of base.

But there seems to be a difference in properties between the double compound of the hydrate of sulphuric acid with the sulphate of the oxide of æthyl and the other sulphovinates. The former is far easier decomposed by water than the latter; but this fact is by no means without analogy. Water is able to decompose many salts of the oxide of antimony, and displace the latter from these combinations as a basic salt; but the combinations of the oxide of antimony with tartaric acid, and other non-volatile organic acids, are not decomposed by water.

According to the earlier method in use, æther was obtained from a mixture of equal parts, by weight, of sulphuric acid and alcohol; here there is more alcohol at the commencement than is requisite. In the progress of the distillation, however, the quantity of sulphuric acid becomes constantly predominating, in proportion as the alcohol passes over as æther; and from the great excess of the hydrate of sulphuric acid, the liberated æther is itself decomposed by the boiling, which in this case takes place at a high temperature, and is then first converted into a double compound of the sulphate of the oxide of æthyl with sulphate of ætherol (oleum vini); and lastly changed by the boiling into olefiant gas, from the presence of too great a quantity of the hydrate of sulphuric acid, and from too high a temperature.

This change of æther into oil of wine and olefiant gas, by an excess of sulphuric acid and too high a temperature, is not the result of a mere deprivation of water, as might be concluded from a comparison of the composition of these substances with that of

æther; for as soon as the slightest trace of oil of wine is evident in the formation of the æther, a corresponding trace of sulphurous acid is disengaged, the quantity of which becomes more considerable if olefiant gas is formed. The production of sulphurous acid stands therefore in definite connexion with that of the oil of wine and olefiant gas. Since the origin of these two bodies takes place only at a high temperature, especially that of the olefiant gas, these substances undoubtedly owe their origin to a similar action of sulphuric acid on æther, as this acid exerts on other bodies of organic origin at high temperatures. The sulphuric acid is coloured black by these, at the high temperature, with the evolution of sulphurous acid and separation of a carbonaceous substance; the same also takes place in the distillation of æther, when continued to the production of oil of wine and olefiant gas.

The origin of this coally matter, which has recently been examined by Erdmann and Lose*, stands therefore in connexion with that of the sulphurous acid, oil of wine, and olefiant gas; consequently the formation of this body is the result of another process, which very likely has nothing to do with the formation of the æther.

When therefore æther is prepared from a mixture of sulphuric acid and alcohol at a very low temperature, it is perfectly free from oil of wine; and, in fact, not a trace of that substance could be observed in the first products which were obtained by the above distillations, not only in those that were performed in the water-bath, but also in those which were carried on at a gentle heat in the sand-bath. Even the last products appeared to be perfectly free from it; but if a considerable quantity of the æthereal liquid was evaporated on blotting-paper, a very slight smell of it might be discovered, a trace however so insignificant, that individuals not well acquainted with the odour of oil of wine could not perceive it. Moreover, when the distillation was at an end, the residuum in the retort was, it is true, of a dark colour, but not deep (*foncé*), so that it resembled a brownish vitriol, such as frequently occurs in commerce; the residue smelt as slightly of sulphurous acid as the distilled æther did of oil of wine. Not a trace of carbonaceous substance was separated. The process by which oil of wine is produced, commences,

* Poggendorff's *Annalen*, vol. xlvii. p. 619.

therefore, in the mixture prepared for the distillation of æther, even at the boiling-point of water, at least when this is long continued; but even then the formation of this body at that temperature is quite trifling in amount.

When æther is distilled from a mixture of sulphuric acid and alcohol in the water-bath, we obtain, as is evident from the above results, less æther than we might expect from the quantity of alcohol employed, and the residue weighs more in proportion. In the last series of experiments described, in which æther was prepared in the water-bath, the residuum, on employing 17 ounces of absolute alcohol and 18 ounces of sulphuric acid, weighed 27 ounces, and the distilled alcoholic æther $4\frac{1}{2}$ ounces; the loss consisted partly in the water distilled, the quantity of which was not determined, in volatilized æther, which in this case volatilized the more, as it was nearly pure, and also in the loss which occurs by pouring out. On employing 1 pound of hydrated alcohol and 1 pound of sulphuric acid, the residuum weighed $26\frac{1}{2}$ ounces, the products 4 ounces and some drachms; the loss consisted partly in the water which passed over, the quantity of which was not accurately determined. In both cases therefore, besides water, æther also remained with the sulphuric acid, undoubtedly as isæthionic acid, probably also in part as æthionic acid. It is very probable that the products which present themselves with æther in a distillation when long continued and at high temperature, are produced, not by the direct decomposition of the æther, but by the decomposition of the isæthionic acid, occasioned by the excess of sulphuric acid and a high temperature; such as the precipitated carbonaceous substance, the sulphurous acid, oil of wine, and lastly, the olefiant gas.

It is well known that the formation of these products is generally avoided in the preparation of æther by the new and most profitable method, in which, as æther passes over, a like quantity of alcohol is allowed to flow into the boiling mixture. The action of an excess of sulphuric acid on the alcohol, or rather on the isæthionic acid, at a high temperature, is thus prevented.

When formerly the production of æther was sought to be explained by the subtraction of the water from it, by means of sulphuric acid, it might with much justice be objected to the present explanation, that other bodies, which have, like sulphu-

ric acid, a great affinity to water, such as the hydrate of potash, chloride of calcium, &c., are not able to transform alcohol into æther; but this objection now falls entirely to the ground, as we know that the æther is not formed by any subtraction of water, but by the decomposition of the sulphovinic acid.

If æther is regarded as a base, then all the theories on the formation of æther are not capable of satisfactorily explaining how a base is discharged from a strongly acid liquid, and by a powerful acid. It is only by the present explanation, and by the analogy which the separation of æther from sulphovinic acid bears to the decomposition of several inorganic salts by means of water, and also by the above-mentioned analogy of æther with a series of oxides which do not, or to a very slight extent, combine with acids, that this phænomenon loses its anomalous appearance.

It seems to me highly desirable in organic chemistry, to illustrate its processes always as much as possible by analogous processes in inorganic chemistry. The greatest advantages have accrued to organic chemistry by the endeavours of Berzelius, Liebig, and Dumas, who have pursued this path, frequently starting, it is true, from very different views.

It is certainly advantageous in so imperfect a science as chemistry, and especially organic chemistry, to ascribe provisionally to a common force all phænomena which stand isolated, for which no suitable analogues can be detected, and which on this account appear wonderful, and thus openly to admit that in the present state of science it is better to avoid explaining a process altogether, than to explain it by some artifice or in a constrained manner. The smaller the number of phænomena which we are compelled to refer to this class, the more perfect the science becomes.

Setting out from this point of view, I have ventured to explain a process in organic chemistry, which has long, and particularly of late years, engaged the attention of chemists, as being analogous to several processes in inorganic chemistry; and if the explanation should not give general satisfaction, the attempt to attain so important an object, will, I trust, meet with approbation.

The present theory is valid, it is true, only for the formation of æther from a mixture of alcohol and sulphuric acid; but quite a similar one may undoubtedly be advanced for the forma-

tion of æther from mixtures of phosphoric and arsenic acids with alcohol. For the present, however, I leave it undecided whether the formation of æther, by treating alcohol with fluoboracic gas, as also with the chloride of zinc and other chlorides, is to be explained by a mere subtraction of water by these substances; or in this way, that they form with alcohol, at the common temperature, combinations analogous to sulphovinic acid, which are decomposed like it, at a high temperature, by the agency of water. The latter view I regard as being the most probable.

Postscript*.

In the preceding Memoir I have compared the formation of æther from a mixture of sulphuric acid and alcohol, with the decomposition of several inorganic salts by means of water; I have endeavoured to show that it is the water which in these cases acts the part of a base, and separates the oxide of æthyl or the metallic oxide, the latter generally as basic salt.

The inorganic salts which I enumerated in this comparison as examples, were those of the oxide of bismuth, the oxide of mercury, and of antimony. These undergo the said decomposition by water even at the common temperature; æther, however, is first separated from a mixture of sulphuric acid and alcohol, or from sulphovinic acid, at a high temperature.

There are, however, among the inorganic weak bases, a considerable number which are eliminated by water, from their combinations with acids only at a high temperature; and the decomposition of the salts of these bases, by means of water, is therefore still more fit to be compared to the formation of æther.

To these bases belongs more especially the peroxide of iron, which is precipitated by water as basic salt from solutions of most of its neutral salts at a high temperature. The weaker the solution of the salt of peroxide of iron, the lower is the temperature which occasions precipitation, and the more completely

* The present Postscript appeared in the following part of Poggendorff's *Annalen*, under the title "On the precipitation of some metallic oxides by water."

is the peroxide of iron thrown down, so that with a certain dilution, as M. Scheerer has shown*, scarcely a trace of the peroxide of iron remains in solution, but the entire quantity is separated as basic salt. As stronger bases are not precipitated by water on boiling, this property of the peroxide of iron has been employed to separate it from the oxides of cobalt, nickel, and other metals†. It may even be separated, by boiling the solution, from alumina, which, although it has with regard to its properties much similarity to the peroxide of iron, is evidently a stronger base; this separation of alumina from the peroxide of iron by means of water at a high temperature, is of some importance to the arts, as in the fabrication of alum the peroxide of iron contained in the mother-liquor is precipitated by mere boiling, and is thus more easy to separate from the alumina than the protoxide of iron, although the former, with sulphuric acid and an alkali, forms an alum which has quite an analogous composition with alumina-alum; and, from being isomorphous with that alum, could crystallize with it in all proportions.

Several other bases have the same property as the peroxide of iron, which like it belong to the class of weaker bases, and also several substances which act as bases towards strong acids, and also as acids towards strong bases, and which on that account are frequently classed among the acids. Among these are the oxide of zirconium, thorina, the peroxide of cerium, peroxide of tin, titanic acid, tellurous acid, columbic acid; also in certain respects molybdic acid, tungstic acid, and vanadic acid. Several combinations of these oxides with acids are soluble in the cold in water, and are precipitated from the solution, on boiling, as oxides or basic salts.

Several of the oxides precipitated in this manner possess, after precipitation by boiling, properties which they do not evince before their solution in acids and precipitation; they are more indifferent than before, are partly of difficult solution in acids, partly insoluble, and do not combine after precipitation with them, even when these are employed in a concentrated state. Titanic acid, peroxide of tin, and many others may be classed here. This peculiarity is in a certain degree ana-

* Poggendorff's *Annalen*, vol. xliv. p. 453. [or Lond. and Edinb. Phil. Mag., vol. xvi. p. 130—Edit.]

† Scheerer in Poggendorff's *Annalen*, vol. xlii. p. 101. [or Lond. and Edinb. Phil. Mag., vol. xvi. p. 131.—Edit.]

logous to that of æther, which, when it has been once separated by boiling from a mixture containing sulphovinic acid, appears not to combine directly with acids.

NOTE.

Our readers will be able to judge how far the theory of ætherification, supported by so much research in the foregoing Memoir, coincides with that previously announced by Professor Graham, in Part II. of his Elements of Chemistry published in 1838, by the following extract from the latter work. Under the head of "*Circumstances which affect the order of decomposition*," the alternate displacement of æther and water by each other, *as bases*, is announced and described by Mr. Graham in the following terms:—

"The remarkable decomposition of alcohol by sulphuric acid, which affords æther, is another similar illustration of decomposition depending upon volatility, and affected by changes in the nature of the atmosphere into which evaporation takes place. Alcohol or the hydrate of æther is added in a gradual manner to sulphuric acid somewhat diluted, and heated to 280°. In these circumstances, the double sulphate of æther and water is formed; water, which was previously combined as base to the acid, being displaced by æther, and evolved together with the water of the alcohol. The first effect of the reaction therefore, is the disengagement of watery vapour, and the creation of an atmosphere of that substance which tends to check its farther evolution. But the existence of such an atmosphere offers a facility for the evaporation of æther, which accordingly escapes from combination with the acid and continues to be replaced by water, the affinity of sulphuric acid for water and for æther being nearly equal, till æther forms such a proportion of the gaseous atmosphere as to check its own evolution, and to favour the evolution of watery vapour. Then again alcohol is decomposed, and more of the double sulphate of water and æther formed as at first; the sulphate of æther of which comes in its turn to be decomposed as before, and æther evolved. Hence, both æther and water distil over in this process, the evolution of one of these bodies favouring the separation and disengagement of the other. In this description, the evolution of water and æther are for the sake of perspicuity supposed to alternate, but it is evident that the result of such an action will be the simultaneous evolution of the two vapours in a certain constant relation to each other." p. 188.

Article XII.

Determination of the Axes of the Elliptic Spheroid of Revolution which most nearly corresponds with the existing Measurements of Arcs of the Meridian. By F. W. Bessel.

[From the *Astronomische Nachrichten*, No. 333.]

The observed latitudes of points on the earth's surface, and the distances between the parallels on which those points are situated, have a relation which would be given by a knowledge of the figure of the earth. If the equation of the earth's surface were known, we should be enabled to determine the constants, whereby the measured distances between the parallels and the observed latitudes of the parallels would be brought into accord within the limits of the errors of observation. But the figure of the earth is not known,—or, rather, we know that it is *irregular*. There is, however, an elliptical spheroid of revolution, the surface of which is not far removed from the surface of the earth at any point; but whether at all points of the respective surfaces this distance may be regarded as a *small* quantity, compared with the ellipticity of the spheroid, is a question yet to be decided by the combination of several measurements of arcs. In the mean time we may make progress in the inquiry by determining the axes of the spheroid which would most nearly represent the existing measurements. If we regard the deviations of the surface of the earth from the surface of the spheroid as following no definite law, their influence on the latitudes is combined with that of the errors of observation of the latitudes, and we must consider that spheroid to be the one sought for, which brings the measured distances between the parallels in correspondence with the latitudes, by correcting the observed latitudes in accordance with the conditions of the method of least squares.

Walbeck first commenced the investigation upon this correct view, but took into account only the most southern and the most northern points of each measured arc, omitting in his calculation the intervening astronomically determined points. Schmidt improved on the earlier calculation, not only by giving proper weight to *all* the observed latitudes, but also by taking

into consideration other measurements of degrees which had been made known in the interim. I return again to the same subject, partly because Schmidt employed several data which appear to me incorrect, partly because I am enabled to avail myself of three additional measurements of arcs. I am indebted for the knowledge of the first to manuscript communications from General von Tenner, who has executed an undertaking of his own of this kind, and has connected it with the northern extremity of von Struve's arc, so that the two together give the measurement of an arc of the meridian of 8° 2′ 29″. I owe the second to manuscript communications of Schumacher, whose measurement includes 1° 31′ 53″. The third, extending over 1° 30′ 29″, has been executed by myself, conjointly with Major Baeyer, in the district of Königsberg: as this is the first public notice of it, I may remark that its more immediate object was to unite the arcs which have been measured in the South and West of Europe with those which have been and will be executed in the North and East; so that a connected chain of triangulation, comprehending the principal European observatories, may extend from Formentera to Finland. The measurement of an arc was combined with this more immediate object, by comparing the latitude of the most southern and most northern points of the triangulation with the latitude of the observatory of Königsberg.

§. 1.

I will first give the data on which the calculation is based, and the sources from whence they are taken.

1. *Peruvian Arc.*

	Latitude.	Amplitude.	Distance between the Parallels.
Tarqui	− 3° 4′ 32″·068	3° 7′ 3″·455	T 176875·5
Cotchesqui	+ 0 2 31·387		

These data rest on the new reductions of the observations by Delambre and von Zach. Delambre, in the *Base du Syst. Métr.* III. p. 133, gives the latitudes

$$-3^\circ\ 4'\ 31''\cdot9 \text{ and } +0^\circ\ 2'\ 31''\cdot22,$$

making the amplitude 3° 7′ 3″·12. Von Zach finds the ampli-

tude 3° 7′ 3″·79 (*Mon. Corresp.* xxvi. p. 52). I have taken the mean of these as the amplitude, and have altered Delambre's latitudes only so much as to bring them into accord with this mean. The distance between the parallels of the two points is found by Delambre = 176877 toises; by von Zach = 176874^{T}. The values employed by Schmidt differ considerably from the above; the amplitude being greater by 5″·205, and the distance less by 9^{T}·33.

2. *First East Indian Arc.*

Trivandeporum .	11° 44′ 52″·590		
Paudree	13 19 49·018	1° 34′ 56″·428	89813·01 T

The account of this measurement is given in the Asiatic Researches, vol. viii. p. 137. The distance is given by Lambton himself = 95721·32 fathoms. But Kater's examination of the standard scale on which the measurement rests, shows that a correction of − 0·000018 must be applied, in order to reduce it to true English measure. The distance thus corrected is = 95719·60 fathoms, which gives the number of toises in the proportion of 1·06576542 to 1.

3. *Second East Indian Arc.*

Punnae	8° 9′ 31″·132		
Putchapolliam ..	10 59 42·276	2° 50′ 11″·144	160944·20 T
Dodagoontah ...	12 59 52·165	4 50 21·033	274694·30
Namthabad	15 5 53·562	6 56 22·430	393828·09
Daumeragidda ..	18 3 16·245	9 53 45·113	561690·06
Takal K'hera ..	21 5 51·532	12 56 20·400	734570·43
Kullianpoor	24 7 11·860	15 57 40·728	906171·67

A part of this great undertaking is described in the Asiatic Researches, vols. x., xii., xiii., and another part in Colonel Everest's account of the measurement of an arc of the meridian, London 1830. It has appeared to me necessary to subject the observations with the zenith sector for determining the latitude to a fresh calculation, which I shall publish in a separate memoir. The data as above are the results of this calculation. The original observations are to be found: for Punnae, Asiat. Res. xii. p. 68; for Putchapolliam, xii. p. 61; for Dodagoontah, x. p. 356; for Namthabad, xii. p. 339; for Daumeragidda, xiii. p. 83; and for Takal K'hera and Kullianpoor, in Everest's Account, &c., pp. 287 and 306. Pages 112—114 of the latter

work contain the distances between the parallels of the astronomically determined points from whence I have deduced

Punnae		
Putchapolliam . . .	171528·76	fathoms.
Dodagoontah . . .	292759·68	„
Namthabad	419728·36	„
Daumeragidda . . .	598629·84	„
Takal K'hera . . .	782879·76	„
Kullianpoor	965766·43	„

The proportion of the toise to the fathom has been given above (2).

4. *French Arc.*

	° ′ ″	° ′ ″	T
Formentera	38 39 56·11		
Montjouy	41 21 44·96	2 41 48·85	153605·77
Barcelona	41 22 47·90	2 42 51·79	154548·9
Carcassonne	43 12 54·30	4 32 58·19	259104·8
Evaux	46 10 42·54	7 30 46·43	427951·5
Pantheon	48 50 49·37	10 10 53·26	580244·6
Dunkirk	51 2 8·85	12 22 12·74	705189·4

The distances of the parallels of the different points from the most southern point are given, with the exception of Barcelona, in the *Base du Syst. Métr.* iii. p. 549. The spot in Barcelona where the astronomical observations were made is $943^{T}\cdot13$ north of Montjouy (ii. p. 565). The latitudes of Formentera, Carcassonne, Evaux, and the Pantheon, are to be found in pages 89 and 459, vol. iii.; in page 89 the latitude of Montjouy is also given. That of Barcelona is (ii. pp. 565 and 615) $= 41° 21' 48''\cdot37 + 59''\cdot53$. For Dunkirk I have taken the result in iii. p. 548. I have followed Delambre's example in leaving out Perpignan, because the observations of the latitude at that place seem less certain than the others. The latitudes taken by Schmidt differ from the above; at Montjouy $+ 0''\cdot49$; at Barcelona $- 0''\cdot74$; Carcassonne $+ 0''\cdot01$; Evaux $- 0''\cdot35$; the Pantheon $0''\cdot43$; Dunkirk $- 0''\cdot11$. He places the parallel of Barcelona $5^{T}\cdot9$ more to the north than is done here.

5. *English Arc.*

	° ′ ″	° ′ ″	T
Dunnose	50 37 7·633		
Greenwich	51 28 39·000	0 51 31·367	49059·89
Blenheim	51 50 27·632	1 13 19·999	69829·19
Arbury Hill	52 13 28·031	1 36 20·398	91696·39
Clifton	53 27 31·130	2 50 23·497	162075·93

These latitudes differ from those given in the Phil. Trans. for 1803. They result from a new combination of the reductions made by General Mudge of his own observations. More particulars on this point, and the reasons which have obliged me to introduce alterations in General Mudge's own data, will be contained in a separate memoir. The distances of the different parallels from *Dunnose*, were originally given in the Phil. Trans. 1803, pp. 441 and 487, as follows:

Greenwich	52282·67 fathoms.
Blenheim	74416·33 „
Arbury Hill	97720·00 „
Clifton	172722·83 „

From Kater's examination of the scale employed in the measurement, these distances require to be multiplied by 0·00007, and will be thereby respectively augmented

3·66; 5·21; 6·84; 12·09.

The distances given above in toises correspond to the distances in fathoms thus augmented.

6. *Hanoverian Arc.*

Göttingen	51° 31′ 47″·85		
Altona	53 32 45·27	2° 0′ 57″·42	T 115163·725

Taken from *Gauss's Breitenunterscheid, &c.*, p. 71.

7. *Danish Arc.*

Lauenburg	53° 22′ 17″·046		
Lysabbel	54 54 10·352	1° 31′ 53″·306	T 87436·538

These results have been communicated to me by *Schumacher*. They might be combined with those of the preceding measurement, if it were not that the latitudes of the two arcs rest on different stars, which would render the combination dependent on the determinations of the declinations of these stars. I think it right to avoid the danger of introducing error into undertakings of such distinguished exactness, by bringing in a foreign element, the more so as Lauenburg, which is $9860^{T}·46$ south of Altona, is $21031^{T}·51$ to the east of the same, forming an angle of nearly 65° with the meridian. The distance of its parallel from that of Altona could not therefore be found with the exactness which is attainable when the inclination to the meridian is less.

8. *Prussian Arc.*

Trunz	54° 13′ 11″·466		T
Königsberg	54 42 50·500	0° 29′ 39″·031	28211·629
Memel	55 43 40·446	1 30 28·980	86176·975

A memoir on this arc is now in the press.

9. *Russian Arc.*

Berlin	52° 2′ 40″·864		T
Nemesch	54 39 4·519	2° 36′ 23″·655	148811·418
Jacobstadt	56 30 4·562	4 27 23·698	254543·454
Bristen	56 34 51·550	4 32 10·686	259110·085
Dorpat	58 22 47·280	6 20 6·416	361824·461
Hochland	60 5 9·771	8 2 28·907	459363·008

These numbers have been communicated to me by General von Tenner. Such of them as refer to Struve's arc—*i. e.*, those for Jacobstadt, Dorpat, and Hochland—agree with the values given in the treatise on the measurement of an arc of latitude in the Baltic provinces of Russia, i. pp. 312 and 338.

10. *Swedish Arc.*

Malörn	65° 31′ 30″·265		T
Pahtawara	67 8 49·830	1° 37′ 19″·565	92777·981

These are *Swanberg's* data, p. 157 of his work. Schmidt has taken the amplitude as $0''\cdot785$ greater, and the distance between the parallels as $17^{T}\cdot251$ less. These differences have been occasioned by two remarks made by *Swanberg* in reducing the observations; first as to what the latitudes would have been, if the density of the air, and with it the refraction, had been assumed as dependent, not, as in the usual manner, on the height of the thermometer, but in a more complicated relation to the same, which certain experiments of Prony's appeared to indicate; and, secondly, as to what the distance would have been inferred from the measurement, if it had been assumed that the double metre, sent from Paris, had its true length at the normal temperature of the toise, viz. 13° Reaumur. As the doubts which gave rise to these two remarks have been fully removed, they ought not to be attended to.

§. 2.

The theory with which the ten above-mentioned arcs should

be compared is as follows. If the two semi-axes of an elliptic spheroid of revolution be designated by a and b, and if

$$\frac{a-b}{a+b}=n,$$

then the length of the arc of the meridian between the equator and the latitude ϕ is the integral

$$s=a\,(1-e^2)\int\frac{d\,\phi}{\sqrt{(1-e^2\sin\phi^2)}};$$

or, developed,

$$s=a\,(1-n)^2\,(1+n)\,\mathrm{N}\,\{\phi-\alpha\sin 2\,\phi+\tfrac{1}{2}\,\alpha'\sin 4\,\phi - \tfrac{1}{3}\,\alpha''\sin 6\,\phi+\ldots\}$$

wherein

$$\mathrm{N}=1+\left(\frac{3}{2}\right)^2 n^2+\left(\frac{3\,.\,5}{2\,.\,4}\right)^2 n^4+\ldots,$$

$$\mathrm{N}\,\alpha=\frac{3}{2}\,n+\frac{3\,.\,5}{2\,.\,4}\,.\,\frac{3}{2}\,n^3+\frac{3\,.\,5\,.\,7}{2\,.\,4\,.\,6}\,.\,\frac{3\,.\,5}{2\,.\,4}\,n^5+\ldots,$$

$$\mathrm{N}\,\alpha'=\frac{3\,.\,5}{2\,.\,4}\,n^2+\frac{3\,.\,5\,.\,7}{2\,.\,4\,.\,6}\,.\,\frac{3}{2}\,n^4+\ldots,$$

$$\mathrm{N}\,\alpha''=\frac{3\,.\,5\,.\,7}{2\,.\,4\,.\,6}\,n^3+\frac{3\,.\,5\,.\,7\,.\,9}{2\,.\,4\,.\,6\,.\,8}\,.\,\frac{3}{2}\,n^5+\ldots$$

and so forth.

If we desire to make this expression dependent on the length of the mean degree of the meridian (g) instead of on the greater semiaxis, we must put $\phi=180°$, whence we obtain

$$180\,g=a\,(1-n)^2\,(1+n)\,\mathrm{N}\,\pi,$$

and thus

$$s=\frac{180\,g}{\pi}\,\{\phi-\alpha\sin 2\,\phi+\tfrac{1}{2}\,\alpha'\sin 4\,\phi-\tfrac{1}{3}\,\alpha''\sin 6\,\phi+\ldots\}.$$

Hence follows the expression for the distance between the parallels corresponding to the latitudes ϕ and ϕ',

$$s'-s=\frac{180\,g}{\pi}\,\{\phi'-\phi-2\,\alpha\sin(\phi'-\phi)\cos(\phi'+\phi) +\tfrac{2}{2}\,\alpha'\sin 2\,(\phi'-\phi)\cos 2\,(\phi'+\phi).$$

If for brevity we write l for the amplitude $\phi'-\phi$, and $2\,\mathrm{L}$ for the sum of the latitudes ϕ' and ϕ, and understand by ω the number $\frac{648000}{\pi}=206264''\!\cdot\!8$, and if we express l in seconds, we thence obtain

$$\frac{3600}{g}\,(s'-s)=l-2\,\omega\,\alpha\sin l\cos 2\,\mathrm{L}+\omega\,\alpha'\sin 2\,l\cos 4\,\mathrm{L} -\tfrac{2}{3}\,\omega\,\alpha''\sin 3\,l\cos 6\,\mathrm{L}+\ldots$$

The problem requires that the observed latitudes $\phi, \phi', \phi'' \ldots$, should be brought in correspondence with the measured distances between the parallels by the application of alterations $x, x', x'' \ldots$, of which the sum of the squares

$$x^2 + x'^2 + x''^2 \ldots$$

shall be a minimum: the values of g and $\alpha, \alpha', \alpha'' \ldots$, which fulfil this condition, belong to the elliptic spheroid of revolution sought. If we write $\phi + x$, and $\phi' + x'$, for ϕ and ϕ', and if we neglect the influence of the alterations on L as well as the squares and products of x and x', the above expression becomes

$$\frac{3600}{g}(s'-s) = l - 2\,\omega\,\alpha \sin l \cos 2\,\mathrm{L} + \omega\,\alpha' \sin 2\,l \cos 4\,\mathrm{L} - \ldots$$
$$+ (x'-x)\,\varrho,$$

where ϱ stands for

$$1 - 2\,\alpha \cos l \cos 2\,\mathrm{L} + 2\,\alpha' \cos 2\,l \cos 4\,\mathrm{L} - \ldots;$$

we have thus

$$x' - x = \frac{1}{\varrho}\left\{\frac{3600}{g}(s'-s) - (l - 2\,\omega\,\alpha \sin l \cos 2\,\mathrm{L}\right.$$
$$\left. + \,\omega\,\alpha' \sin 2\,l \cos 4\,\mathrm{L} \ldots)\right\},$$

and we must now so determine g and the compression as to fulfil the above-named condition.

If we take g_1 and α_1 as the approximate values of g and α, make

$$g = \frac{g_1}{1+i}; \qquad \alpha = \alpha_1\,(1+k);$$

and if we neglect the squares and products of i and k, the expression for $x' - x$ becomes

$$\frac{1}{\varrho}\left\{\frac{3600}{g_1}(s'-s) - l\right\} \quad \frac{\omega}{\varrho}(2\,\alpha_1 \sin l \cos 2\,\mathrm{L} - \alpha_1' \sin 2\,l \cos 4\,\mathrm{L} + \ldots),$$
$$+ \frac{1}{\varrho}\cdot\frac{3600}{g_1}(s'-s)\,i + \frac{\omega}{\varrho}(2\,\alpha_1 \sin l \cos 2\,\mathrm{L} - \alpha_1 \frac{d\alpha'_1}{d\alpha_1} \sin 2\,l \cos 4\,\mathrm{L} + \ldots)\,k,$$

in which α'_1 and $\alpha_1 \frac{d\alpha'_1}{d\alpha_1}$ expressed by α_1 are respectively

$$\frac{5}{6}\alpha_1^2 + \frac{25}{162}\alpha'^4_1, \quad \text{and} \; \frac{5}{3}\,\alpha_1^2 + \frac{50}{81}\,\alpha_1^4.$$

Then, if we make

$$m = \frac{1}{\varrho}\left\{\frac{3600}{g_1}(s'-s) - l\right\} + \frac{\omega}{\varrho}\left\{2\,\alpha_1 \sin l \cos 2\,\mathrm{L}\right.$$
$$\left. - \left(\frac{5}{6}\alpha_1^2 + \frac{25}{162}\alpha_1^4\right) \sin 2\,l \cos 4\,\mathrm{L}\right\},$$

$$a = \frac{1}{\varrho} \cdot \frac{3600}{g_1} (s' - s),$$

$$b = \frac{\omega}{\varrho} \left\{ 2\alpha_1 \sin l \cos 2\,\mathrm{L} - \left(\frac{5}{3} \alpha_1{}^2 + \frac{50}{81} \alpha_1{}^4 \right) \sin 2l \cos 4\,\mathrm{L} \right\}$$

we have

$$x' - x = m + a\,i + b\,k,$$

and a similar equation for the combination of the southernmost point of an arc with each of the points to the north of it.

The sum of the squares of the alterations to be applied to all the latitudes of an arc is thus:

$$x^2 + (m + a\,i + b\,k + x)^2 + (m' + a'\,i + b'\,k + x)^2, \text{ \&c.};$$

for other arcs the sums are

$$x_1{}^2 + (m_1 + a_1\,i + b_1\,k + x_1)^2 + (m'_1 + a'_1\,i + b'_1 k + x_1)^2, \text{ \&c.},$$

$$x_2{}^2 + (m_2 + a_2\,i + b_2\,k + x_2)^2 + m'_2 + a'_2\,i + b'_2\,k + x_2)^2, \text{ \&c.},$$

$$\text{\&c.}, \qquad \text{\&c.}, \qquad \text{\&c.},$$

each of these gives thus for the determination of its own value of x the equation

$$0 = \mu\,x + (m) + (a)\,i + (b)\,k,$$

in which μ is the number of the observed latitudes, and (m) (a) and (b) denote as in the usual notation of Gauss. It furnishes also towards the determination of i and k, which must be founded on all the existing measured arcs, the following contributions:

$$(a\,m) + (a)\,x + (a^2)\,i + (a\,b)\,k,$$

$$(b\,m) + (b)\,x + (a\,b)\,i + (b^2)\,k,$$

which, eliminating x, become

$$(a\,m) - \frac{(a)\,(m)}{\mu} + \left\{ (a^2) - \frac{(a)\,(a)}{\mu} \right\} i + \left\{ (a\,b) - \frac{(a)\,(b)}{\mu} \right\} k,$$

$$(b\,m) - \frac{(b)\,(m)}{\mu} + \left\{ (a\,b) - \frac{(a)\,(b)}{\mu} \right\} i + \left\{ (b^2) \quad \frac{(b)\,(b)}{\mu} \right\} k,$$

the sums of the first as well as of the second of these contributions, so furnished by all the existing measured arcs, being made $= 0$, give the two equations necessary for the determination of i and k.

§. 3.

I will now communicate the *several* equations of condition which I have deduced from each of the ten arcs on which this examination is founded. My view in so doing is to obtain the advantage of being able to avail myself of any subsequent

alterations of the results assumed as observed, for the purpose of correcting my calculation without being obliged to repeat it throughout. In order to avoid unnecessary multiplication of figures, I will assume the unknown quantities sought, instead of i and k, to be $10000\, i = p$ and $10\, k = q$. I set out from the assumption that

$$g = \frac{57008^{\mathrm{T}}}{1+i}, \qquad \alpha = \frac{1+k}{400}.$$

1. *Peruvian Arc.*

$$x_1^1 - x_1 = +1''{\cdot}966 + 1{\cdot}1225\, p + 5{\cdot}6059\, .\, q.$$

2. *First East Indian Arc.*

$$x_2^1 - x_2 = +0''{\cdot}937 + 0{\cdot}5697\, p + 2{\cdot}5835\, .\, q.$$

3. *Second East Indian Arc.*

$$\begin{aligned}
x_3^1 - x_3 &= +0''{\cdot}455 + 1{\cdot}0212\, p + 4{\cdot}8270\, .\, q, \\
x_3^2 - x_3 &= +6{\cdot}681 + 1{\cdot}7428\, p + 8{\cdot}1250\, .\, q, \\
x_3^3 - x_3 &= +1{\cdot}745 + 2{\cdot}4983\, p + 11{\cdot}4652\, .\, q, \\
x_3^4 - x_3 &= +3{\cdot}878 + 3{\cdot}5624\, p + 15{\cdot}9264\, .\, q, \\
x_3^5 - x_3 &= +8{\cdot}272 + 4{\cdot}6585\, p + 20{\cdot}1840\, .\, q, \\
x_3^6 - x_3 &= +2{\cdot}677 + 5{\cdot}7458\, p + 24{\cdot}0262\, .\, q.
\end{aligned}$$

4. *French Arc.*

$$\begin{aligned}
x_4^1 - x_4 &= -0''{\cdot}297 + 0{\cdot}9709\, p + 0{\cdot}8601\, .\, q, \\
x_4^2 - x_4 &= -3{\cdot}641 + 0{\cdot}9768\, p + 0{\cdot}8642\, .\, q, \\
x_4^3 - x_4 &= -4{\cdot}259 + 1{\cdot}6374\, p + 1{\cdot}1889\, .\, q, \\
x_4^4 - x_4 &= -9{\cdot}319 + 2{\cdot}7037\, p + 1{\cdot}2671\, .\, q, \\
x_4^5 - x_4 &= -3{\cdot}092 + 3{\cdot}6651\, p + 0{\cdot}8659\, .\, q, \\
x_4^6 - x_4 &= +0{\cdot}889 + 4{\cdot}4533\, p + 0{\cdot}2051\, .\, p.
\end{aligned}$$

5. *English Arc.*

$$\begin{aligned}
x_5^1 - x_5 &= +3''{\cdot}504 + 0{\cdot}3095\, p - 0{\cdot}3178\, .\, q, \\
x_5^2 - x_5 &= +4{\cdot}937 + 0{\cdot}4405\, p - 0{\cdot}4658\, .\, q, \\
x_5^3 - x_5 &= +3{\cdot}758 + 0{\cdot}5784\, p - 0{\cdot}6308\, .\, q, \\
x_5^4 - x_5 &= -0{\cdot}892 + 1{\cdot}0223\, p - 1{\cdot}2226\, .\, q.
\end{aligned}$$

6. *Hanoverian Arc.*

$$x_6^1 - x_6 = +5''{\cdot}679 + 0{\cdot}7263\, p - 0{\cdot}9294\, .\, q.$$

7. *Danish Arc.*

$$x_7^1 - x_7 = -0''{\cdot}369 + 0{\cdot}5513\, p - 0{\cdot}8537\, .\, q.$$

8. *Prussian Arc.*

$$x_8^1 - x_8 = -0''{\cdot}368 + 0{\cdot}1779\,p - 0{\cdot}2852\,.\,q,$$
$$x_8^2 - x_8 = +3{\cdot}790 + 0{\cdot}5433\,p - 0{\cdot}9157\,.\,q.$$

9. *Russian Arc.*

$$x_9^1 - x_9 = +0''{\cdot}248 + 0{\cdot}9384\,p - 1{\cdot}3293\,.\,q,$$
$$x_9^2 - x_9 = +5{\cdot}110 + 1{\cdot}6049\,p - 2{\cdot}5184\,.\,q,$$
$$x_9^3 - x_9 = +5{\cdot}939 + 1{\cdot}6337\,p - 2{\cdot}5741\,.\,q,$$
$$x_9^4 - x_9 = +2{\cdot}909 + 2{\cdot}2809\,p - 3{\cdot}9289\,.\,q,$$
$$x_9^5 - x_9 = +5{\cdot}276 + 2{\cdot}8953\,p - 5{\cdot}3824\,.\,q.$$

10. *Swedish Arc.*

$$x'_{10} - x_{10} = 0''\,507 + 0{\cdot}5839\,p - 1{\cdot}9711\,.\,q.$$

From these equations of condition I have obtained for each of the measured arcs the sums marked (m), (a), (b), $(a\,m)$, $(a\,a)$, &c.

	(m)	(a)	(b)	$(a\,m)$	$(a\,a)$	$(a\,b)$	$(b\,m)$	$(b\,b)$
1	+ 1·966	+ 1·1225	+ 5·6059	+ 2·2068	1·2600	+ 6·2926	+ 11·0211	31·4261
2	+ 0·937	0·5697	+ 2·5835	+ 0·5338	0·3246	+ 1·4718	+ 2·4207	6·6745
3	+ 23·708	19·2290	+ 84·5538	+ 84·1994	77·7283	+ 336·5465	+ 369·5289	1459·0687
4	− 19·719	14·4072	+ 5·2513	− 43·3870	45·1527	+ 11·1389	− 22·7680	5·2976
5	+ 11·307	2·3507	− 2·6370	+ 4·5209	1·6694	− 1·9183	− 4·6932	2·2105
6	+ 5·679	0·7263	− 0·9294	+ 4·1247	0·5275	− 0·6750	− 5·2780	0·8638
7	− 0·369	0·5513	− 0·8537	− 0·2034	0·3039	− 0·4706	+ 0·3150	0·7288
8	+ 3·422	0·7212	− 1·2009	+ 1·9936	0·3268	− 0·5482	− 3·3655	0·9198
9	+ 19·482	9·3532	− 15·7331	+ 40·0169	19·7106	− 31·0396	− 68·3130	59·1418
10	− 0·507	0·5839	− 1·9711	− 0·2960	0·3409	− 1·1509	+ 0·9994	3·8852

After the elimination of x_1, x_2, x_3, we have the data furnished by different arcs for the equations serving to determine p and q.

	$(a\,m_1)$	$(a\,a_1)$	$(a\,b_1)$	$(b\,m_1)$	$(b\,b_1)$
1	+ 1·1034	0·6300	+ 3·1463	+ 5·5106	15·7131
2	+ 0·2669	0·1623	+ 0·7359	+ 1·2104	3·3373
3	+ 19·0734	24·8940	+ 104·2771	+ 83·1572	437·7342
4	− 2·8019	15·5002	+ 0·3308	− 7·9757	1·3582
5	− 0·7950	0·5642	− 0·6785	+ 1·2701	0·8197
6	+ 2·0624	0·2638	− 0·3375	− 2·6390	0·4319
7	− 0·1017	0·1519	− 0·2353	+ 0·1575	0·3644
8	+ 1·1710	0·1534	− 0·2595	− 1·9957	0·4391
9	+ 9·6768	5·1302	− 9·5138	− 17·2270	17·8868
10	− 0·1480	0·1705	− 0·5755	+ 0·4997	1·9426
Sums	+ 29·5073	47·6205	+ 96·8900	+ 61·9681	480·0273

Thus we have for the determination of p and q the equations

$$0 = +29{\cdot}5073 + 47{\cdot}6205\,p + 96{\cdot}8900\,q,$$
$$0 = +61{\cdot}9681 + 96{\cdot}8900\,p + 480{\cdot}0273\,q,$$

from the solution of which we obtain

$$p = -0{\cdot}60574; \qquad \text{Weight} = \ \ 28{\cdot}064$$
$$q = -0{\cdot}0068280; \qquad \text{,,} \quad = 282{\cdot}892.$$

§. 4.

If we compare the several observed latitudes with this determination of p and q, we obtain the alterations required to make them agree with the elliptic spheroid of revolution, to which the found values of those quantities belong.

$$\begin{aligned} x_1 &= -0''{\cdot}624 \\ x_1^1 &= +0{\cdot}624 \end{aligned}$$

$$\begin{aligned} x_2 &= -0{\cdot}287 \\ x_2^1 &= +0{\cdot}287 \end{aligned}$$

$$\begin{aligned} x_3 &= -1{\cdot}640 \\ x_3^1 &= -1{\cdot}837 \\ x_3^2 &= +3{\cdot}929 \\ x_3^3 &= -1{\cdot}487 \\ x_3^4 &= -0{\cdot}029 \\ x_3^5 &= +3{\cdot}672 \\ x_3^6 &= -2{\cdot}608 \end{aligned}$$

$$\begin{aligned} x_4 &= +4{\cdot}069 \\ x_4^1 &= +3{\cdot}178 \\ x_4^2 &= -0{\cdot}170 \\ x_4^3 &= -1{\cdot}190 \\ x_4^4 &= -6{\cdot}897 \\ x_4^5 &= -1{\cdot}249 \\ x_4^6 &= +2{\cdot}259 \end{aligned}$$

$$\begin{aligned} x_5 &= -1''{\cdot}980 \\ x_5^1 &= +1{\cdot}338 \\ x_5^2 &= +2{\cdot}793 \\ x_5^3 &= +1{\cdot}432 \\ x_5^4 &= -3{\cdot}483 \end{aligned}$$

$$\begin{aligned} x^6 &= -2{\cdot}623 \\ x_6^1 &= +2{\cdot}623 \end{aligned}$$

$$\begin{aligned} x_7 &= +0{\cdot}349 \\ x_7^1 &= -0{\cdot}349 \end{aligned}$$

$$\begin{aligned} x_8 &= -0{\cdot}998 \\ x_8^1 &= -1{\cdot}472 \\ x_8^2 &= +2{\cdot}469 \end{aligned}$$

$$\begin{aligned} x_9 &= -2{\cdot}321 \\ x_9^1 &= -2{\cdot}632 \\ x_9^2 &= +1{\cdot}834 \\ x_9^3 & \quad +2{\cdot}646 \\ x_9^4 & \quad -0{\cdot}766 \\ x_9^5 & \quad +1{\cdot}238 \end{aligned}$$

$$\begin{aligned} x_{10} &= +0{\cdot}424 \\ x_{10}^1 &= -0{\cdot}424 \end{aligned}$$

The sum of the squares of these alterations is $= 203{\cdot}391$, and the mean value of each of them

$$= \sqrt{\frac{203{\cdot}391}{38-10}} = \pm 2''{\cdot}695.$$

From this determination, and from the above given weights of the determinations of p and q, the mean errors of these quantities are

$$= \pm 0{\cdot}5087 \text{ and } = \pm 0{\cdot}1602,$$

or the mean errors

$$\text{of } i = \pm 0{\cdot}00005087,$$
$$\text{of } k = \pm 0{\cdot}01602;$$

thus we have

$$g = \frac{57008}{1-0{\cdot}000060574} = 57011^{\mathrm{T}}{\cdot}453 \,.\, \overbrace{\pm\, 2^{\mathrm{T}}{\cdot}900}^{\text{M. error}}$$

$$\alpha = \frac{1-0{\cdot}0006828}{400} = 0{\cdot}002498293 \,.\, \pm\, 0{\cdot}00004002$$

§. 5.

I have further only to seek for the two axes of the elliptic spheroid of revolution, and to develop the numerical values belonging to it of some formulæ which are of frequent application.

The reversion of the series (§. 2)

$$\alpha = \frac{\frac{3}{2}n + \frac{45}{16}n^3 + \frac{525}{128}n^5 + \ldots\ldots}{1 + \frac{9}{4}n^2 + \frac{225}{64}n^4 + \ldots\ldots}$$

gives the expression of n in terms of α, namely

$$n = \frac{2}{3}\alpha + \frac{1}{9}\alpha^3 + \frac{23}{486}\alpha^5 + \ldots,$$

and by substitution of the found values of α,

$$n = \frac{a-b}{a+b} = 0{\cdot}0016655304.$$

Hence we obtain the proportion between the axes of the elliptic spheroid of revolution, which corresponds most nearly to the arcs which are under examination,

$$\frac{1}{2\,n} + \frac{1}{2} : \frac{1}{2\,n} - \frac{1}{2} = 300{\cdot}7047 : 299{\cdot}7047;\ \text{mean error}$$
$$= \pm\, 4{\cdot}81.$$

Further, we obtain the axes themselves according to the formulæ given in §. 2.

$$a = \frac{180\,g}{\pi\,(1-n)^2\,(1+n)\,\mathrm{N}}$$

$$b \quad \frac{180\,g}{\pi\,(1+n)^2\,(1-n)\,\mathrm{N}};$$

or, numerically expressed,

$$a = 327\overset{\mathrm{T}}{1}953{\cdot}854 \qquad \log a = 6\,.\,5148071699$$
$$b = 3261072{\cdot}900 \qquad \log b = 6\,.\,5133605073.$$

The length of the quadrant of the meridian, which according to the original view, ought to be 10,000,000 metres, is according to this determination,

$$90\,g \,.\, \frac{864}{443{\cdot}296} = 10000565^{\mathrm{m}}{\cdot}278.$$

Its mean uncertainty is $= 508^{m}\cdot 7$, which is almost equal to its difference from the round number. Hence we see how uncertain the length of the metre would have been, even now that the number of measured arcs has been considerably augmented, if its original definition as the 10,000,000th part of the quadrant of the meridian had been adhered to. Its uncertainty would still amount to at least $0^{L}\cdot 0225$, a quantity which could only be deemed insignificant in very rough measurements.

The formulæ of which I have to give the numerical development are the following:

1. The length of a degree of the meridian of which the mean latitude $= \phi$:

$$m = 57011^{T}\cdot 453 - 284^{T}\cdot 851 \cos 2\phi + 0^{T}\cdot 593 \cos 4\phi - 0^{T}\cdot 001 \cos 6\phi.$$

2. The length of a degree of the parallel:

$$p = 57153^{T}\cdot 885 \cos \phi - 47^{T}\cdot 576 \cos 3\phi + 0^{T}\cdot 059 \cos 5\phi,$$

or if

$$\sin \psi = e \sin \phi, \ldots (\log e = 8\cdot 9110835),$$

then

$$\log p = 4\cdot 7566845\cdot 4 + \log \cos \phi - \log \cos \psi.$$

3. Let the radius of curvature in the meridian $= r'$, in the direction perpendicular to it $= r''$, in the azimuth $\alpha = r$:

$$\frac{\omega}{r'} = 0''\cdot 06314600 + 0''\cdot 00031552 \cos 2\phi + 0''\cdot 00000013 \cos 4\phi,$$

$$\frac{\omega}{r''} = 0''\cdot 06293548 + 0''\cdot 00010482 \cos 2\phi - 0''\cdot 00000004 \cos 4\phi$$

or

$$\log \frac{\omega}{r'} = 8\cdot 8025112 . 9 + 3 \log \cos \psi$$

$$\log \frac{\omega}{r''} = 8\cdot 7996179 . 6 + \log \cos \psi,$$

and

$$\frac{\omega}{r} = \lambda + \lambda' \cos 2\alpha,$$

wherein

$$\lambda = 0''\cdot 06304074 + 0''\cdot 00021017 \cos 2\phi + 0''\cdot 00000004 \cos 4\phi,$$
$$\lambda' = 0\ \cdot 00010526 + 0\cdot 00010535 \cos 2\phi + 0\ \cdot 00000009 \cos 4\phi.$$

4. Let the distance from the centre of the earth $= \varrho$, and the corrected latitude $= \phi'$:

$$\log . \varrho \cos \phi' = \log . \cos \phi - \log \cos \psi,$$
$$\log . \varrho \sin \phi' = \log . \sin \phi - \log \cos \psi - 0\cdot 0028933 . 3.$$

BESSEL.

Article XIII.

The Galvanic Circuit investigated Mathematically. By Dr. G. S. Ohm*.

Preface.

I HEREWITH present to the public a theory of galvanic electricity, as a special part of electrical science in general, and shall successively, as time, inclination, and means permit, arrange more such portions together into a whole, if this first essay shall in some degree repay the sacrifices it has cost me. The circumstances in which I have hitherto been placed, have not been adapted either to encourage me in the pursuit of novelties, or to enable me to become acquainted with works relating to the same department of literature throughout its whole extent. I have therefore chosen for my first attempt a portion in which I have the least to apprehend competition. May the well-disposed reader receive the performance with the same love for the object as that with which it is sent forth.

The Author.

Berlin, May 1st, 1827.

Introduction.

The design of this Memoir is to deduce strictly from a few principles, obtained chiefly by experiment, the rationale of those electrical phænomena which are produced by the mutual contact of two or more bodies, and which have been termed Galvanic:—its aim is attained if by means of it the variety of facts be presented as unity to the mind. To begin with the most simple investigations, I have confined myself at the outset to those cases where the excited electricity propagates itself only in one dimension. They form, as it were, the scaffold to a greater structure, and contain precisely that portion, the more accurate knowledge of which may be gained from the elements of natural philosophy, and which, also, on account of its greater accessibility, may be given in a more strict form. To answer

* "*Die Galvanische Kette mathematisch bearbeitet von Dr. G. S. Ohm: Berlin*, 1827." Translated from the German by Mr. William Francis, Student in Philosophy in the University of Berlin.

this especial purpose, and at the same time as an introduction to the subject itself, I give, as a forerunner of the compressed mathematical investigation, a more free, but not on that account less connected, general view of the process and its results.

Three laws, of which the first expresses the mode of distribution of the electricity within one and the same body, the second the mode of dispersion of the electricity in the surrounding atmosphere, and the third the mode of appearance of the electricity at the place of contact of two heterogeneous bodies, form the basis of the entire Memoir, and at the same time contain everything that does not lay claim to being completely established. The two latter are purely experimental laws; but the first, from its nature, is, at least partly, theoretical.

With regard to this first law, I have started from the supposition that the communication of the electricity from one particle takes place directly only to the one next to it, so that no immediate transition from that particle to any other situate at a greater distance occurs. The magnitude of the transition between two adjacent particles, under otherwise exactly similar circumstances, I have assumed as being proportional to the difference of the electric forces existing in the two particles; just as, in the theory of heat, the transition of caloric between two particles is regarded as proportional to the difference of their temperatures. It will thus be seen that I have deviated from the hitherto usual mode of considering molecular actions introduced by Laplace; and I trust that the path I have struck into will recommend itself by its generality, simplicity, and clearness, as well as by the light which it throws upon the character of former methods.

With respect to the dispersion of electricity in the atmosphere, I have retained the law deduced from experiments by Coulomb, according to which, the loss of electricity, in a body surrounded by air, in a given time, is in proportion to the force of the electricity, and to a coefficient dependent on the nature of the atmosphere. A simple comparison of the circumstances under which Coulomb performed his experiments, with those at present known respecting the propagation of electricity, showed, however, that in galvanic phænomena the influence of the atmosphere may almost always be disregarded. In Coulomb's experiments, for instance, the electricity driven to the surface of the

body was engaged in its entire expanse in the process of dispersion in the atmosphere; while in the galvanic circuit the electricity almost constantly passes through the interior of the bodies, and consequently only the smallest portion can enter into mutual action with the air; so that, in this case, the dispersion can comparatively be but very inconsiderable. This consequence, deduced from the nature of the circumstances, is confirmed by experiment; in it lies the reason why the second law seldom comes into consideration.

The mode in which electricity makes its appearance at the place of contact of two different bodies, or the electrical tension of these bodies, I have thus expressed: when dissimilar bodies touch one another, they constantly maintain at the point of contact the same difference between their electroscopic forces.

With the help of these three fundamental positions, the conditions to which the propagation of electricity in bodies of any kind and form is subjected may be stated. The form and treatment of the differential equations thus obtained are so similar to those given for the propagation of heat by Fourier and Poisson, that even if there existed no other reasons, we might with perfect justice draw the conclusion that there exists an intimate connexion between both natural phænomena; and this relation of identity increases, the further we pursue it. These researches belong to the most difficult in mathematics, and on that account can only gradually obtain general admission; it is therefore a fortunate chance, that in a not unimportant part of the propagation of electricity, in consequence of its peculiar nature, those difficulties almost entirely disappear. To place this portion before the public is the object of the present memoir, and therefore so many only of the complex cases have been admitted as seemed requisite to render the transition apparent.

The nature and form commonly given to galvanic apparatus favours the propagation of the electricity only in one dimension; and the velocity of its diffusion combined with the constantly acting source of galvanic electricity is the cause of the galvanic phænomena assuming, for the most part, a character which does not vary with time. These two conditions, to which most frequently galvanic phænomena are subjected, viz. change of the electric state in a single dimension, and its independency of time,

are however precisely the reasons why the investigation is brought to a degree of simplicity which is not surpassed in any branch of natural philosophy, and is altogether adapted to secure incontrovertibly to mathematics the possession of a new field of physics, from which it had hitherto remained almost totally excluded.

The chemical changes which so frequently occur in some, generally fluid, portions of a galvanic circuit, greatly deprive the result of its natural simplicity, and conceal, to a considerable extent, by the complications they produce, the peculiar progression of the phænomenon; they are the cause of an unexampled change of the phænomenon, which gives rise to so many apparent exceptions to the rule, frequently even to contradictions, in so far as the sense of this word is itself not in contradiction to nature. I have distinctly separated the consideration of such galvanic circuits in which no portion undergoes a chemical change, from those whose activity is disturbed by chemical action, and have devoted a separate part to the latter in the Appendix. This total separation of two parts forming a whole, and, as might appear, the less dignified position of the latter, will find in the following circumstance a sufficient explanation. A theory, which lays claim to the name of an enduring and fruitful one, must have all its consequences in accordance with observation and experiment. This, it seems to me, is sufficiently established with respect to the first of the parts above-mentioned, partly by the previous experiments of others, and partly by some performed by myself, which first made me acquainted with the theory here developed, and subsequently rendered me entirely devoted to it. Such is not the case with regard to the second part. A more accurate experimental verification is in this case almost entirely wanting, to undertake which I need both the requisite time and means; and therefore I have merely placed it in a corner, from which, if worth the trouble, it may be drawn hereafter, and may then also be further matured under better nursing.

By means of the first and third fundamental positions we obtain a distinct insight into the primary galvanic phænomenon in the following way. Imagine, for instance, a ring everywhere of equal thickness and homogeneous, having, at any one place, in its whole thickness, one and the same electrical tension, *i. e.* inequality in the electrical state of two surfaces situated close to each other, from which causes, when they have come

into action, and the equilibrium is consequently disturbed, the electricity will, in its endeavour to re-establish itself, if its mobility be solely confined to the extent of the ring, flow off on both sides. If this tension were merely momentary, the equilibrium would very soon be re-established; but if the tension is permanent, the equilibrium can never be restored; but the electricity, by virtue of its expansive force, which is not sensibly restrained, produces in a space of time, the duration of which almost always escapes our senses, a state which comes nearest to that of equilibrium, and consists in this; that by the constant transmission of the electricity, a perceptible change in the electric condition of the parts of the body through which the current passes is nowhere produced. The peculiarity of this state, also occurring frequently in the transmission of light and heat, has its foundation in this; that each particle of the body situated in the circle of action receives in each moment just so much of the transmitted electricity from the one side as it gives off to the other, and therefore constantly retains the same quantity. Now since by reason of the first fundamental position the electrical transition only takes place directly from the one particle to the other, and is, under otherwise similar circumstances, determined according to its energy by the electrical difference of the two particles, this state must evidently indicate itself on the ring, uniformly excited in its entire thickness, and similarly constituted in all its parts, by a constant change of the electric condition, originating from the point of excitation, proceeding uniformly through the whole ring, and finally again returning to the place of excitation; whilst at this place itself, a sudden spring in the electric condition, constituting the tension, is, as was previously stated, constantly perceptible. In this simple separation or division of the electricity lies the key to the most varied phænomena.

The mode of separation of the electricity has been completely determined by the preceding observation; but the absolute force of the electricity at the various parts of the ring still remains uncertain. This property may be best conceived, by imagining the ring, without its nature being altered, opened at the point of excitation and extended in a straight line, and representing the force of the electricity at each point by the length of a perpendicular line erected upon it; that directed upwards may represent a positive electrical, but that downwards a negative

electrical, state of the part. The line A B (Plate XXIV., fig. 1) may accordingly represent the ring extended in a straight line, and the lines A F and B G perpendicular to A B may indicate by their lengths the force of the positive electricities situated at the extremities A and B. If now the straight line F G be drawn from F to G, also F H parallel to A B, the position of F G will give the mode of separation of the electricity, and the quantities B G − A F or G H the tension occurring at the extremities of the ring; and the force of the electricity at any other place C, may easily be expressed by the length of C D drawn through C perpendicularly to A B. But, from the nature of the galvanic excitation, merely the quantity of the tension or the length of the line G H, therefore the difference of the lines A F and B G, is determined, but not at all the absolute magnitudes of the lines A F and B G; consequently the mode of separation may be represented quite as well by any other line parallel to the former, *e. g.* by I K, for which the tension still constantly retains the same value expressed by K N, because the ordinates situated at present below A B assume a relation opposed to their former one. Which of the infinitely numerous lines parallel to F G would express the actual state of the ring cannot be stated in general, but must in each case be separately determined from the circumstances which occur. Moreover, it is easily conceived that, as the position of the line sought is given, it would be completely determined for one single part of the ring by the determination of any one of its points, or, in other words, by the knowledge of the electric force. If, for instance, the ring lost all its electricity by abduction at the place C, the line L M drawn through C parallel to F G would in this case express with perfect certainty the electrical state of the ring. This variability in the separation of the electricity is the source of the changeableness of the phænomenon peculiar to the galvanic circuit. I may further add, that it is evidently quite indifferent whether the position of the line F G with respect to that of A B be fixed; or whether the position of the line F G remain constantly the same, and the position of A B with respect to it be altered. The latter course is by far the more simple where the separation of the electricity assumes a more complex form.

The conclusions just arrived at, which hold for a ring homogeneous throughout its whole extent, may easily be ex-

tended to a ring composed of any number of heterogeneous parts, if each part be of itself homogeneous and of the same thickness. I may here take as an example of this extension a ring composed of two heterogeneous parts. Let this ring be imagined as before open at one of its places of excitation and stretched out to form the right line A B C (fig. 2), so that A B and B C indicate the two heterogeneous parts of the ring. The perpendiculars A F, B G, will represent by their lengths the electrical forces present at the extremities of the part A B; on the other hand, B H and C I, those present at the extremities of the part B C; accordingly A F + C I or F K will represent the tension at the opened place of excitation, and G H the tension occurring at B at the point of contact. Now if we only bear in mind the permanent state of the circuit, the straight lines F G and H I will, from the reasons above mentioned, indicate by their position the mode of separation of the electricity in the ring; but whether the line A C will keep its place, or must be advanced further up or down, remains uncertain, and can only be found out in each distinct case by other separate considerations. If, for instance, the point O of the circuit is touched abductively, and thus deprived of all electricity, O N would disappear; and therefore the line L M drawn through N parallel with A C would in this case give the position of A C required. It is hence evident, how sometimes this, sometimes another, position of the line A C in the figure F G H I, representing the separation of the electricity, may be the one suited to the circumstances; and herein we recognise the source of the variability of galvanic phænomena already mentioned.

It is, however, essentially requisite, in order to be able to judge thoroughly of the present case, to attend to a circumstance the mention of which has hitherto been purposely avoided, that the various considerations might be separated as distinctly as possible. The distances F K and G H are indeed given by the tensions existing at the two places of excitation, but the figure F G H I is not yet wholly determined by this alone. For instance, the points G and H might move down towards G′ and H′, so that G′ H′ would equal G H, giving rise to the figure F G′ H′ I, which would indicate quite a different mode of separation of the electricity, although the individual tensions in it still retain their former magnitude.

Consequently if that which has been stated with respect to the circuit of two members is to acquire a sense no longer subject to any arbitrary explanation, this uncertainty must be removed. The first fundamental law effects this in the following way:—For since the state of the ring alone, independent of the time, is regarded, each section must, as has already been stated, receive in every moment the same quantity of electricity from one side as it gives off to the other. This condition occasions upon such portions of the ring as have perfectly the same constitution at their various points, the constant and uniform change in the separation which is represented in the first figure by the straight line F G, and in the second by the straight lines F G and H I. But when the geometrical or the physical nature of the ring changes in passing from one of its component parts to another, the reason of this constancy and uniformity no longer obtains; consequently the manner in which the several straight lines are combined into a complete figure must first be deduced from other considerations. To facilitate the object, I will separately consider the geometrical and physical difference of the single parts, each independently.

Let us first suppose that every section of the part B C is m times smaller than in the part A B, while both parts are composed of the same substance; the electric state of the ring, which is independent of time, and which requires that everywhere throughout the entire ring just as much electricity be received on one side as is given off from the other, can evidently only exist under the condition that the electric transition from one particle to the other in the same time within the portion B C is m times greater than in the portion A B; because it is only in this manner that the action in both parts can maintain equilibrium. But in order to produce this m times greater transition of the electricity from element to element, the electrical difference of element to element within the portion B C must, according to the first fundamental position, be m times greater in the portion A B; or when this determination is transferred to the figure, the line H I must sink m times more on equal portions, or have an m times greater "dip" than the line F G. By the expression "dip" (*Gefälle*), is to be understood the difference of such ordinates which belong to two places distant one unit of length from each other. From this consideration results the following rule: *The dips of*

the lines F G *and* H I *in the portions* A B *and* B C, *composed of like substance, will be inversely to each other as the areas of the sections of these parts.* By this the figure F G H I is now fully determined.

When the parts A B and B C of the ring have the same section but are composed of different substances, the transition of the electricity will then no longer be dependent solely on the progressive change of electricity in each part from element to element, but at the same time also on the peculiar nature of each substance. This difference in the distribution of the electricity, caused solely by the material nature of the bodies, whether it have its origin in the peculiar structure or in any other peculiar state of the bodies to electricity, establishes a distinction in the electrical conductibility of the various bodies; and even the present case may afford some information respecting the actual existence of such a distinction and give rise to its more accurate determination. In fact, since the ring composed of the two parts A B and B C differs from the homogeneous one only in this respect, that the two parts are formed of two different substances, a difference in the dip of the two lines F G and H I will make known a difference in the conductibility of the two substances, and one may serve to determine the other. In this way we arrive at the following position, supplying the place of a definition: *In a ring consisting of two parts* A B *and* B C, *of like sections but formed of different substances, the dips of the lines* F G *and* H I *are inversely as the conducting powers of the two parts.* If we have once ascertained the conducting powers of the various substances, they may be employed to determine the dips of the lines F G and H I in every case that may occur. By this, then, the figure F G H I is entirely determined. The determination of the conductibility from the separation of the electricity is rendered very difficult from the weak intensity of galvanic electricity, and from the imperfection of the requisite apparatus; subsequently we shall obtain a more easy means of effecting this purpose.

From these two particular cases we may now ascend in the usual way to the general one, where the two prismatic parts of the ring neither possess the same section nor are constituted of the same substance. *In this case the dips of the two parts must be in the inverse ratio of the products of the sections and powers of conduction.* We are hereby enabled to deter-

mine completely the figure F G H I in every case, and also to distinguish perfectly the mode of electrical separation in the ring. All the peculiarities, hitherto considered separately, of the ring composed of two heterogeneous parts, may be summed up in the following manner: *In a galvanic circuit consisting of two heterogeneous prismatic parts, there takes place in regard to its electrical state a sudden transition from the one part to the other at each point of excitation, forming the tension there occurring, and from one extremity of each point to the other a gradual and uniform transition; and the dips of these two transitions are inversely proportional to the products of the conductibilities and sections of each part.*

Proceeding in this manner, we are able without much difficulty to inquire into the electrical state of a ring composed of three or more heterogeneous parts, and to arrive at the following general law: *In a galvanic circuit consisting of any indefinite number of prismatic parts, there takes place in regard to its electrical state at each place of excitation a sudden transition, from one part to the other, forming the tension there prevailing, and within each part a gradual and uniform transition from the one extremity to the other; and the dips of the various transitions are inversely proportional to the products of the conductibilities and sections of each part.* From this law may easily be deduced the entire figure of the separation for each particular case, as I will now show by an example.

Let A B C D (fig. 3) be a ring composed of three heterogeneous parts, open at one of its places of excitation, and extended in a straight line. The straight lines F G, H I, K L represent by their position the mode of separation of the electricity in each individual part of the ring, and the lines A F, B G, B H, C I, C K, and D E drawn through A, B, C and D perpendicular to A D such quantities that G H, K I and L M or D L − A F show by their length the magnitude of the tensions occurring at the individual places of excitation. From the known magnitude of these tensions, and from the given nature of the single parts A B, B C, and C D, the figure of the electrical separation has to be entirely determined.

If we draw straight lines parallel to A D, through the points F, H and K, meeting the line drawn through B, C and D perpendicular to A D, in the points F′, H′, K′, then according to what has already been demonstrated, the lines G F′, I H′ and

L K′ are directly proportional to the lengths of the parts A B, B C and C D, and inversely proportional to the products of the conductibility and section of the same part, consequently the relations of the lines G F′, I H′ and L K′ to each other are given. Further, that G F′ + I H′ + L K′ = G H − K I + (D L − A F = L M) is also known, as the tensions represented by G H, K I and D L − A F are given. From the given relations of the lines G F′, I H′, L K′ and their known sum, these lines may now be found individually; the figure F G H I K L is evidently then entirely determined. But the position of this figure with respect to the line A D remains from its very nature still undecided.

If we recollect, that proceeding in the same direction A D, the tensions represented by G H and D L − A F or L M indicate a sudden sinking of the electric force at the respective places of excitation, that represented by I K on the contrary a sudden rise of the force; and that tensions of the first kind are regarded and treated as positive quantities, while tensions of the latter kind are considered as negative quantities, we find the above example lead us to the following generally valid rule: *If we divide the sum of all the tensions of the ring composed of several parts into the same number of portions which are directly proportional to the lengths of the parts and inversely proportional to the products of their conductibilities and their sections, these portions will give in succession the amount of gradation which must be assigned to the straight lines belonging to the single parts and representing the separation of the electricity; at the same time the positive sum of all the tensions indicates a general rise, on the contrary the negative sum of all the tensions a general depression of those lines.*

I will now proceed to the determination of the electric force at any given position in every galvanic circuit, and here again I shall lay down as basis fig. 3. For this purpose let a, a', a'' indicate the tensions existing at B, C, and between A and D, so that in this case also a and a'' represent additive, a' on the contrary a subtractive line, and λ, λ', λ'' any lines which are directly as the lengths of the parts A B, B C, and C D, and inversely as the products of the conductibilities and sections of the same parts; further, let

$$a + a' + a'' = \mathrm{A}$$

and

$$\lambda + \lambda' + \lambda'' = L$$

then according to the law just ascertained

G F′ is a fourth proportional to L, A and λ
I H′ a fourth proportional to L, A and λ'
L K′ a fourth proportional to L, A and λ''.

Draw the line F M through F parallel to A D, regard this line as the axis of the abscissæ, and erect at any given points X, X′, X″ the ordinates X Y, X′ Y′, X″ Y″, we obtain their respective values, thus:

In the first place we have, since A B = F F″

$$AB : GF' = FX : XY,$$

whence follows:

$$XY = \frac{FX \,.\, GF'}{AB},$$

or if we substitute for G F′ its value $\frac{A \,.\, \lambda}{L}$

$$XY = \frac{A}{L} \cdot \frac{FX \,.\, \lambda}{AB}.$$

If now x represent a line such that

$$AB : FX = \lambda : x,$$

then

$$XY = \frac{A}{L} \,.\, x.$$

Secondly, since B C and F′ X′ are equal to the lines drawn through I and Y′ to G H parallel to A D

$$BC : IH' = F'X' : F'H - X'Y',$$

whence

$$- X'Y' = \frac{IH' \,.\, F'X'}{BC} - F'H$$

or, since F′ H = G H − G F′

$$- X'Y' = \frac{IH' \,.\, F'X'}{BC} + GF' - a.$$

If now for I H′ and G F′ we substitute their values $\frac{A \,.\, \lambda'}{L}$ and $\frac{A \,.\, \lambda}{L}$, we obtain

$$- X'Y' = \frac{A}{L} \left(\lambda + \frac{F'X' \,.\, \lambda'}{BC} \right) - a;$$

and if by x' we represent a line such that

$$BC : F'X' = \lambda' : x',$$

then

$$-X'Y' = \frac{A}{L}(\lambda + x') - a.$$

Thirdly, since $CD = KK'$ and $F''X''$ is equal to the part of KK' which extends from K to the line $X''Y''$, we have

$$CD : LK' = F''X'' : X''Y'' - KF'',$$

whence

$$X''Y'' = \frac{LK' . F''X''}{CD} + KF'',$$

or, since $KF'' = KI + IH' - F'H$ and $F'H = GH - GF'$,

$$X''Y'' = \frac{LK' . F''X''}{CD} + IH' + GF' - (a + a').$$

If now for LK', IH', GF' we substitute their values

$\frac{A . \lambda''}{L}$, $\frac{A . \lambda'}{L}$, $\frac{A . \lambda}{L}$, we obtain

$$X''Y'' = \frac{A}{L}\left(\lambda + \lambda' + \frac{F''X'' . \lambda''}{CD}\right) - (a + a');$$

and if by x'' we represent a line such that

$$CD : F''X'' = \lambda'' : x''$$

we have

$$X''Y'' = \frac{A}{L}(\lambda + \lambda' + x'') - (a + a').$$

These values of the ordinates, belonging to the three distinct parts of the circuit and different in form from each other, may be reduced as follows to a common expression. For if F is taken as the origin of the abscissæ, FX will be the abscissa corresponding to the ordinate XY which belongs to the homogeneous part AB of the ring, and x will represent the length corresponding to this abscissa in the reduced proportion of $AB : \lambda$. In like manner FX' is the abscissa corresponding to the ordinate $X'Y'$ which is composed of the parts FF' and $F'X'$ belonging to the homogeneous portions of the ring, and λ, x' are the lengths reduced in the proportions of $AB : \lambda$ and $BC : \lambda'$ corresponding to these parts. Lastly FX'' is the abscissa corresponding to the ordinate $X''Y''$, which is composed of the parts FF', $F'F''$, $F''X''$ belonging to the homogeneous portions of the ring, and λ, λ', x'' are the lengths reduced in the proportions of $AB : \lambda$, $BC : \lambda'$, $CD : \lambda''$. If in consequence of this consideration we call the values x, $\lambda + x'$, $\lambda + \lambda' + x''$

reduced abscissæ and represent them generally by y, we obtain

$$X\,Y = \frac{A}{L} \cdot y$$

$$-X'\,Y' = \frac{A}{L} \cdot y - a$$

$$X''\,Y'' = \frac{A}{L} \cdot y - (a + a'),$$

and it is evident that L is the same in reference to the whole length A D or F M as y is to the lengths F X, F X′, F X″, on account of which L is termed the entire reduced length of the circuit. Further, if we consider that for the abscissa corresponding to the ordinate X Y the tension has experienced no abrupt change, but that for the abscissa corresponding to the ordinate X′ Y′ the tension has experienced the abrupt changes a, a'; and if we represent generally by O the sum of all the abrupt changes of the tensions for the abscissa corresponding to the ordinate y, then all the values found for the various ordinates are contained in the following expression:

$$\frac{A}{L} \cdot y - O.$$

But these ordinates express, when an arbitrary constant, corresponding to the length A F, is added to them, the electric forces existing at the various parts of the ring. If therefore we represent the electric force at any place generally by u we obtain the following equation for its determination:

$$u = \frac{A}{L}\, y - O + c,$$

in which c represents an arbitrary constant. This equation is generally true, and may be thus expressed in words: *The force of the electricity at any place of a galvanic circuit composed of several parts, is ascertained by finding the fourth proportional to the reduced length of the entire circuit, the reduced length of the part belonging to the abscissa, and the sum of all the tensions, and by increasing or diminishing the difference between this quantity and the sum of all the abrupt changes of tension for the given abscissa by an arbitrary quantity which is constant for all parts of the circuit.*

When the determination of the electric force at each place of the circuit has been effected, it only remains to determine the magnitude of the electric current. Now in a galvanic circuit of

the kind hitherto mentioned, the quantity of electricity passing through a section of it in a given time is everywhere the same, because at all places and in each moment the same quantity in the section leaves it on the one side as enters it from the other, but in different circuits this quantity may be very different: therefore, in order to compare the actions of several galvanic circuits *inter se*, it is requisite to have an accurate determination of this quantity, by which the magnitude of the current in the circuit is measured. This determination may be deduced from figure 3 in the following manner. It has already been shown that the force of the electric transition in each instant from one element to the adjacent one is given by the electric difference between the two existing at that time, and by a magnitude dependent upon the kind and form of the particles of the body, viz. the conductibility of the body. But the electrical difference of the elements in the part B C, for instance, reduced to a constant unit of distance, will be expressed by the dip of the line H I or by the quotient $\frac{\mathrm{I\,H'}}{\mathrm{B\,C}}$; if, therefore, we now indicate by $\varkappa$ the magnitude of the conductibility of the part B C,

$$\frac{\varkappa \,.\, \mathrm{I\,H'}}{\mathrm{B\,C}}$$

will express the force of the transition from element to element, or the *intensity* of the current in the part B C; consequently if ω represent the magnitude of the section in the part B C, the quantity of electricity passing in each instant from one section to the adjacent one, or the *magnitude* of the current, will be expressed by

$$\frac{\varkappa \,.\, \omega \,.\, \mathrm{I\,H'}}{\mathrm{B\,C}};$$

or if S represent this magnitude of the current, we have

$$\mathrm{S} = \frac{\varkappa \,.\, \omega \,.\, \mathrm{I\,H'}}{\mathrm{B\,C}},$$

and if we substitute for I H′ its value $\frac{\mathrm{A} \,.\, \lambda'}{\mathrm{L}}$

$$\mathrm{S} = \frac{\mathrm{A}}{\mathrm{L}} \cdot \frac{\varkappa \,.\, \omega \,.\, \lambda'}{\mathrm{B\,C}}.$$

Hitherto the letters λ, λ', λ'' have represented lines which are proportional to the quotients formed of the lengths of the parts A B, B C, C D, and the products of their conductibilities and their sections. If we restrict for the present this determination,

which leaves the absolute magnitude of the lines $\lambda, \lambda', \lambda''$ uncertain, so that the magnitudes $\lambda, \lambda', \lambda''$ shall not be merely proportional to the said quotients, but shall be likewise equal to them, and henceforth vary this limitation in accordance with the meaning of the expression "reduced lengths," the first of the two preceding equations becomes

$$S = \frac{I\,H'}{\lambda'},$$

which gives the following generally: *The magnitude of the current in any homogeneous portion of the circuit is equal to the quotient of the difference between the electrical forces present at the extremities of this portion divided by its reduced length.* This expression for the forces of the current will be continued to be employed subsequently. The second of the former equations passes, by the adopted change, into

$$S = \frac{A}{L},$$

which is generally true, and already reveals the equality of the force of the current at all parts of the circuit; in words it may be thus expressed: *The force of the current in a galvanic circuit is directly as the sum of all the tensions, and inversely as the entire reduced length of the circuit,* bearing in mind that at present by reduced length is understood the sum of all the quotients obtained by dividing the actual lengths corresponding to the homogeneous parts by the product of the corresponding conductibilities and sections.

From the equation determining the force of the current in a galvanic circuit in conjunction with the one previously found, by which the electric force at each place of the circuit is given, may be deduced with ease and certainty all the phænomena belonging to the galvanic circuit. The former I had already some time ago derived from manifoldly varied experiments* with an apparatus which allows of an accuracy and certainty of measurement not suspected in this department; the latter expresses all the observations pertaining to it, which already exist in great number, with the greatest fidelity, which also continues where the equation leads to results no longer comprised in the circle of previously published experiments. Both proceed uninterruptedly hand in hand with nature, as I now hope to

* Schweigger's *Jahrbuch*, 1826, part 2.

demonstrate by a short statement of their consequences; at the same time I consider it necessary to observe, that both equations refer to all possible galvanic circuits whose state is permanent, consequently they comprise the voltaic combination as a particular case, so that the theory of the pile needs no separate comment. In order to be distinct, I shall constantly, instead of employing the equation $u = \frac{A}{L} y - O + c$, only take the third figure, and therefore will merely remark here, once for all, that all the consequences drawn from it hold generally.

In the next place, the circumstance that the separation of the electricity, diffusing itself over the galvanic circuit, maintains at the different places a permanent and unchangeable gradation, although the force of the electricity is variable at one and the same place, deserves a closer inspection. This is the reason of that magic mutability of the phænomena which admits of our predetermining at pleasure the action of a given place of the galvanic circuit on the electrometer, and enables us to produce it instantly. To explain this peculiarity I will return to figure 3. Since the figure of separation F G H I K L, is always wholly determined from the nature of any circuit; but its position with respect to the circuit A D, as was seen, is fixed by no inherent cause, but can assume any change produced by a movement common to all its points in the direction of the ordinates, the electrical condition of each point of the circuit expressed by the mutual position of the two lines, may be varied constantly, and at will, by external influences. When, for example, A D is at any time the position representing the actual state of the circuit, so that, therefore, the ordinate S Y″ expresses by its length the force of the electricity at the place of the circuit to which that ordinate belongs, then the electrical force corresponding to the point A, at the same time will be represented by the line A F. If now the point A be touched abductively, and thus be entirely deprived of all its force, the line A D will be brought into the position F M, and the force previously existing in the point S will be expressed by the length X″ Y″; this force, therefore, has suddenly undergone a change, corresponding to the length S X″. The same change would have occurred if the circuit had been touched abductively at the point

Z, because the ordinate Z W is equal to that of A F. If the circuit were touched at the place where the two parts A B and B C join, but so that the contact was made within the part B C, we should have to imagine A D advanced to N O; the electrical force at the point S would in this case be increased to the force indicated by T Y″. But if the contact took place, still at the same point, viz. where the parts A B and B C touch each other, but within the part A B, the line A D would be moved to P Q, and the force belonging to the point S would sink to the negative force expressed by U Y″. If, lastly, the pile had been touched abductively at the point D, we should have prescribed for the line A D the position R L, and the electrical force at the point S would have assumed the negative force indicated by V Y″. The law of these changes is obvious, and may be expressed generally thus: *each place of a galvanic circuit undergoes mediately, in regard to its outwardly acting electrical force, the same change which is produced immediately at any other place of the circuit by external influences.*

Since each place of a galvanic circuit undergoes, of itself, the same change to which a single place was compelled, the change in the quantity of electricity, extending over the whole circuit, is proportional, on the one hand, to the sum of all the places, *i. e.* to the space over which the electricity is diffused in the circuit, and moreover, to the change in the electric force produced at one of these places. From this simple law result the following distinct phænomena. If we call r the space over which the electricity is diffused in the galvanic circuit, and imagine this circuit touched at any one place by a non-conducting body, and designate by u the electric force at this place before contact, by u that after contact, the change produced in the force at this place is $u_1 - u$; consequently the change of the whole quantity of electricity in the circuit is $(u_1 - u)\, r$. If, now, we suppose that the electricity in the touched body is diffused over the space R, and is at all places of equal strength, and, at the same time, that at the place of contact itself the circuit and the body possess the same electric force, viz. u, it it is evident u R will be the quantity of electricity imparted to the body, and

$$(u_1 - u)\, r = u\, \mathrm{R},$$

whence we obtain

$$u = \frac{u_{,} r}{r + \mathrm{R}}.$$

The intensity of the electricity received by the body will, therefore, be the more nearly equal to that which the circuit possessed at the place of contact before being touched, the smaller R *is with respect to* r; *it will amount to the half when* $\mathrm{R} = r$, *and become weaker, as* R *becomes greater in comparison with* r. Since these changes are merely dependent on the relative magnitude of the spaces r and R, and not at all on the qualitative nature of the circuit, they are merely determined by the dimensions of the circuit, nay, even by foreign masses brought into conducting connexion with the circuit. If we connect this fact with the theory of the condensor, we arrive at an explanation of all the relations of the galvanic circuit to the condensor, noticed by Jäger*, which is perfectly surprising. I content myself with regard to this point to refer to the memoir itself, to give room here for the insertion of some new peculiarities of the galvanic circuit.

The mode of separation of the electricity, within a homogeneous part of the circuit, is determined by the magnitudes of the dips of the lines F G, H I, K L, (fig. 3,) and there again by the magnitudes of the ratios $\frac{\mathrm{GF}'}{\mathrm{AB}}$, $\frac{\mathrm{IH}'}{\mathrm{BC}}$, $\frac{\mathrm{LK}'}{\mathrm{CD}}$. But, as was already shown,

$$\mathrm{GF}' = \frac{\mathrm{A}}{\mathrm{L}} . \lambda, \quad \mathrm{IH}' = \frac{\mathrm{A}}{\mathrm{L}} . \lambda', \quad \mathrm{LK}' = \frac{\mathrm{A}}{\mathrm{L}} . \lambda'';$$

hence it may be seen, without much trouble, that the magnitude of the dip of the line corresponding to any part of the circuit, and representing the separation of the electricity, is obtained by multiplying the value $\frac{\mathrm{A}}{\mathrm{L}}$ by the ratio of the reduced to the actual length of the same part. If, therefore, (λ) represent the reduced length of any homogeneous part of the circuit and (l) its actual length, the magnitude of the dip of the straight line belonging to this part, and representing the separation of the electricity, is

$$\frac{\mathrm{A}}{\mathrm{L}} . \frac{(\lambda)}{(l)},$$

which expression, if we designate by $(\varkappa)$ the conductibility,

* Gilbert's *Annalen*, vol. xiii.

and by (ω) the section of the same part, may also be written thus:

$$\frac{A}{L} \cdot \frac{(\lambda)}{(\varkappa)\,(\omega)}.$$

This expression leads to a more detailed knowledge of the separation of the electricity in a galvanic circuit. For since A and L designate values which remain identical for each part of the same circuit, it is evident *that the dips in the separate homogeneous parts of a circuit are to one another inversely as the products of the conductibility, and the section of the part.* If consequently a part of the circuit surpasses all others from the circumstance, that the product of its conductibility and its section is far smaller than in the others, it will be the most adapted to reveal, by the magnitude of its dip, the differences of the electric force at its various points. If its actual length is, at the same time, not inferior to those of the other parts, its reduced length will far surpass those of the other parts; and it is easily conceived that such a relation between the various parts can be brought about, that even its reduced length may remain far greater than the sum of the reduced lengths of all the other parts. But in this case the reduced length of this one part is nearly equal to the reduced lengths of the entire circuit, so that we may substitute, without committing any great error, $\frac{(l)}{(\varkappa)\,(\omega)}$ for L, if (l) represent the actual length of the said part, $(\varkappa)$ its conductibility, and (ω) its section; but then the dip of this part changes nearly into

$$\frac{A}{(l)},$$

whence it follows *that the difference of the electrical forces at the extremities of this part is nearly equal to the sum of all the tensions existing in the circuit.* All the tensions seem, as it were, to tend towards this one part, causing the electrical separation to appear in it with otherwise unusual energy, when all the tensions, or, at least, the greater part in number and magnitude, are of the same kind. In this way the scarcely perceptible gradation in the separation of the electricity, in a closed circuit, may be rendered distinctly evident, which, otherwise, would not be the case without a condensor, on account of the weak intensity of galvanic forces. This remarkable pro-

perty of galvanic circuits, representing, as it were, their entire nature, had already been noticed long ago in various bad conducting bodies, and its origin sought for in their peculiar constitution *; I have, however, enumerated in a letter to the editor of the *Annalen der Physik* †, the conditions under which this property of the galvanic circuit may be observed, even in the best conductors, the metals; and the necessary precautions, founded on experience, by which the success of the experiment is assured, described in it, are in perfect accordance with the present considerations.

The expression $\frac{A}{L} \cdot \frac{(\lambda)}{(l)}$, denoting the dip of any portion of the circuit, vanishes when L is indefinitely great, while A and $\frac{(\lambda)}{(l)}$ retain finite values. Consequently, if L assumes an indefinitely great value, while A remains finite, the dip of the straight lines representing the separation of the electricity, in all such parts of the circuit, whose reduced length has a finite ratio to the actual length, vanishes, or what comes to the same thing, the electricity is of equal force at all places of each such part. Now, since L represents the sum of the reduced lengths of all the parts of the circuit, and these reduced lengths evidently can only assume positive values, L becomes indefinite as soon as one of the reduced lengths assumes an infinite value. Further, since the reduced length of any part represents the quotient obtained by dividing the actual length by the product of the conductibility and the section of the same part, it becomes infinite when the conductibility of this part vanishes, *i. e.* when this part is a non-conductor of electricity. *When, therefore, a part of the circuit is a non-conductor, the electricity expands uniformly over each of the other parts, and its change from one part to the other is equal to the whole tension there situated.* This separation of the electricity, relative to the open circuit, is far more simple than that in the closed circuit, which has hitherto formed the object of our consideration, as is geometrically represented by the lines F G, H I, K L, (fig. 3) taking a position parallel to A D. It distinctly demonstrates *that the difference between the electrical forces, occurring at any two*

* Gilbert's *Annalen*, vol. viii. pp. 205, 207, and 456. Vol. x. p. 11.
† Jahrgang, 1826. Part v. p. 117.

places of the circuit, is equal to the sum of all the tensions situated between these two places, and consequently increases or decreases exactly in the same proportion as this sum. When, therefore, one of these places is touched abductively, the sum of all the tensions, situated between the two, makes its appearance at the other place, at the same time the direction of the tensions must always be determined by an advance from the latter place. All the experiments on the open pile, with the help of the electroscope, instituted at such length by Ritter, Erman, and Jäger, and described in Gilbert's *Annalen**, are expressed in this last law.

All the electroscopic actions of a galvanic circuit of the kind, described at the outset, have been above stated; I therefore pass at present to the consideration of the current originating in the circuit, the nature of which, as explained above, is expressed at every place of the circuit by the equation

$$S = \frac{A}{L}.$$

Both the form of this equation, as well as the mode by which we arrive at it, show directly *that the magnitude of the current in such a galvanic circuit remains the same at all places of the circuit, and is solely dependent on the mode of separation of the electricity, so that it does not vary, even though the electric force at any place of the circuit be changed by abductive contact, or in any other way.* This equality of the current at all places of the circuit has been proved by the experiments of Becquerel†, and its independency of the electric force at any determinate place of the circuit by those of G. Bischof‡. An abduction or adduction does not alter the current of the galvanic circuit so long as they only act immediately on a single place of the circuit; but if two different places were acted upon contemporaneously, a second current would be formed, which would necessarily, according to circumstances, more or less change the first.

The equation

$$S = \frac{A}{L}$$

shows that the current of a galvanic circuit is subjected to a

* Vol. viii., xii., and xiii.

† *Bulletin universel. Physique.* Mai, 1825.

‡ Kastner's *Archiv*, vol. iv. Part 1.

change, by each variation originating either in the magnitude of a tension or in the reduced length of a part, which latter is itself again determined, both by the actual length of the part, as well as by its conductibility and by its section. This variety of change may be limited, by supposing only one of the enumerated elements to be variable, and all the remainder constant. We thus arrive at distinct forms of the general equation corresponding to each particular instance of the general capability of change of a circuit. To render the meaning of this phrase evident by an example, I will suppose that in the circuit only the actual length of a single part is subjected to a continual change; but that all the other values denoting the magnitude of the current remain constantly the same, and, consequently, also in its equation. If we designate by x this variable length, and the conductibility corresponding to the same part by $\varkappa$, its section by ω, and the sum of the reduced lengths of all the others by Λ, so that $L = \Lambda + \frac{x}{\varkappa \cdot \omega}$, then the general expression for the current changes into the following:

$$S = \frac{A}{\Lambda + \frac{x}{\varkappa \cdot \omega}};$$

or if we multiply both the numerator and denominator by $\varkappa\,\omega$, and substitute a for $\varkappa\,\omega\,\Lambda$, and b for $\varkappa\,\omega\,\Lambda$, into the following:

$$S = \frac{a}{b + x},$$

where a and b represent two constant magnitudes, and x the variable length of a portion of the circuit fully determined with respect to its substance and its section. This form of the general equation, in which all the invariable elements have been reduced to the smallest number of constants, is that which I had practically deduced from experiments to which the theory here developed owes its origin*. The law which it expresses relative to the length of conductors, differs essentially from that which Davy formerly, and Becquerel more recently, were led to by experiments; it also differs very considerably from that advanced by Barlow, as well as from that which I had previously drawn from other experiments, although the two latter come much nearer to the truth. The first, in fact,

* See Schweigger's *Jahrbuch*, 1826. Part 2.

is nothing more than a formula of interpolation, which is valid only for a relatively very short variable part of the entire circuit, and, nevertheless, is still applicable in very different possible modes of conduction, which is already evident, from its merely admitting the variable portion of the circuit, and leaving out of consideration all the other part; but all partake in common of this evil, that they have admitted a foreign source of variability, produced by the chemical change of the fluid portion of the circuit, of which I shall speak more fully hereafter. I have already treated, in other places, more at length of the relations of the various forms of the law to one another.

From the numerous separate peculiarities of the galvanic circuit resulting from the general equation

$$S = \frac{A}{L},$$

I will here merely mention a few. It is immediately evident that a change in the arrangement of the parts has no influence on the magnitude of the current if the sum of the tensions be not affected by it. Nor is the magnitude of the current altered, when the sum of the tensions, and the entire reduced length of the circuit, change in the same proportion; consequently a circuit, the sum of whose tensions is very small in comparison to that of another circuit, may still produce a current, which, in energy, may be equal to that in the other circuit, when merely that which it loses in force of tensions is replaced by a shortening of its reduced length. *In this circumstance is the source of the peculiar difference between thermo- and hydro-circuits.* In the former only metals occur as parts of the circuit; in the latter, besides the metals, aqueous fluids, whose power of conduction, in comparison to that of the metals, is exceedingly small; on which account the reduced lengths of the fluid surpass, beyond all proportion, those of the metallic parts, with in all respects equal dimensions, and even remain considerably greater when diminished by shortening their actual lengths, and increasing their sections, so long, at least, as this diminution is not carried too far. And thence it is that the reduced length of the thermo-circuit is, in general, far smaller than that of the hydro-circuit, whence we may infer a tension smaller in the same proportion in the former, although the magnitude of the current,

in the thermo-circuit, cedes in nothing to that in the hydro-circuit. *The great difference between a thermo- and hydro-circuit, both of which produce a current of the same energy, is evident when the same change is made on both, as will be shown in the following consideration.* Let the reduced length of a thermo-circuit be L, and the sum of its tensions A, the reduced length of an hydro-circuit m L, and the sum of its tensions m A, then the magnitude of the current in the former is expressed by $\frac{A}{L}$, in the latter by $\frac{m\,A}{m\,L}$, and is consequently the same in both circuits. But this equality of the current no longer exists if the same new part λ of the reduced length be introduced into both, for then the magnitude of the current is in the first

$$\frac{A}{L + \lambda},$$

in the second

$$\frac{m\,A}{m\,L + \lambda}.$$

If we connect with this determination an evaluation, even if merely superficial, of the quantities m, L, and λ, we shall readily be convinced that in cases where the simple hydro-circuit can still produce in the part λ actions of heat or chemical decomposition, the simple thermo-circuit may not possess the hundredth, and in some cases not the thousandth part of the requisite force, whence the absence of similar effects in it is easily to be understood. We are also able to understand why a diminution of the reduced lengths of the thermo-circuit (by increasing, for instance, the section of the metals constituting it) cannot give rise to the production of those effects, although the magnitude of its current may be increased by this means to a higher degree than in the hydro-circuit producing such effects. This difference in the conductibility of metallic bodies and aqueous fluids, is the cause of a peculiarity noticed with respect to hydro-circuits, which it is here, perhaps, the proper place to mention. Under the usual circumstances, the reduced length of the fluid portion is so large, in comparison to that of the metallic portion, that the latter may be overlooked, and the former alone taken instead of the reduced length of the entire circuit; but then the magnitude of the current in circuits which have the same tension is in the inverse ratio to the reduced length of the fluid

portion. Consequently, if merely such circuits are compared in which the fluid parts have the same actual lengths and the same conductibilities, *then the magnitude of the current in these circuits is in direct ratio to the section of the fluid portion.* However, it must not be overlooked, that a more complex definition must take the place of this simple one when the reduced length of the metallic portion can no longer be regarded as evanescent towards that of the fluid, which case occurs whenever the metallic portion is very long and thin, or the fluid portion is a good conductor, and with unusually large terminal surfaces.

From the equation

$$S = \frac{A}{L}$$

we can easily perceive that, when a portion is taken from the galvanic circuit, and is replaced by another, and after this change the sum of the tensions as well as the energy of the current still remains perfectly the same, these two parts have the same reduced length, *consequently their actual lengths are as the products of their conductibilities and sections. The actual lengths of such parts are therefore, when they have like sections, as their conductibilities, and when they have like conductibilities as their sections.* By the first of these two relations we are enabled to determine the conductibilities of various bodies in a far more advantageous manner than by the previously mentioned process, and it has already been employed by Becquerel and myself for several metals*. The second relation may serve to demonstrate experimentally the independence of the effect on the form of the section, as has previously been done by Davy, and recently by myself†.

In the voltaic pile, the sum of the tensions, and the reduced length of the simple circuit, is repeated as frequently as the number of elements of which it consists expresses. If, therefore, we designate by A the sum of all the tensions in the simple circuit, by L its reduced length, and by n the number of elements in the pile, the magnitude of the current in the closed pile is evidently

* *Bulletin universel. Physique*, Mai 1825, and Schweigger's *Jahrbuch*, 1826. Part 2.

† Gilbert's *Annalen*, nn. Folge. Vol. xi. p. 253, and Schweigger's *Jahrbuch*, 1827.

$$\frac{n\,A}{n\,L},$$

while in the simple closed circuit it is

$$\frac{A}{L}.$$

If we now introduce into the simple circuit, as well as into the pile, one and the same new part Λ of the reduced length, upon which the current is to act, the magnitude of the current thus altered in the simple circuit will be

$$\frac{A}{L + \Lambda},$$

and in the voltaic pile

$$\frac{n\,A}{n\,L + \Lambda}, \quad \text{or} \quad \frac{A}{L + \frac{\Lambda}{n}}.$$

It is hence evident *that the current is constantly greater in a voltaic pile than in the simple circuit, but it is merely imperceptibly greater so long as* Λ *is very small in comparison with* L; *on the contrary, this increase approximates the nearer to n times, the greater* Λ *becomes to* n L, *and consequently the more so in comparison with* L. Besides this mode of increasing the magnitude of the galvanic current, there is a second one, which consists in shortening the reduced lengths of the simple circuit, which may be effected by increasing its section, or placing several simple circuits by the side of each other, and connecting them in such a way that together they only form one single simple circuit. If we now retain the same signs, so that

$$\frac{A}{L + \Lambda}$$

again denotes the magnitude of the current in one element, then, in the above-mentioned combination of n elements into a single circuit, the magnitude of the current is evidently

$$\frac{A}{\frac{L}{n} + \Lambda}, \quad \text{or} \quad \frac{n\,A}{L + n\,\Lambda},$$

which indicates a slight increase in the action of the new combination when Λ *is very great in comparison with* L; *on the contrary, a very powerful one when* Λ *is very small in comparison with* $\frac{L}{n}$, *and consequently the more so in comparison with* L. It hence

follows that the one combination is most active in those cases where the other ceases to be so, and *vice versâ*. If therefore we are in possession of a certain number of simple circuits intended to act upon the portion whose reduced length is Λ, much depends on the way in which they are placed, in order to produce the greatest effect of current; whether all be side by side, or all in succession, or whether part be placed by the side of each other, and part in series. It may be mathematically shown that it is most advantageous to form them into a voltaic combination, of so many equal parts, that the square of this number be equal to the quotient $\frac{\Lambda}{L}$. When $\frac{\Lambda}{L}$ is equal to, or smaller than Λ, they had best be arranged by the side of each other, and in succession when $\frac{\Lambda}{L}$ is equal to, or larger than the square of the number of all the elements. *We see in this determination the reason why in most cases a simple circuit, or at least a voltaic combination of only a few simple circuits, is sufficient to produce the greatest effect.* If we bear in mind, that since the quantity of the current is the same at all places of the circuit, its intensity at the various places must be in inverse proportion to the magnitude of the section there situated, and if we grant that the magnetic and chemical effects, as well as the phænomena of light and heat in the circuit, are direct indications of the electrical current, and that their energy is determined by that of the current itself, then a detailed analysis of the current, here indicated merely in outline, will lead to the perfect explanation of the numerous and partially enigmatical anomalies observed in the galvanic circuit, in as far as we are justified in considering the physical nature of the circuit as invariable*. Those great differences which are frequently met with in the statements of various observers, and which are not consequences of the dimensions of their different apparatus, have undoubtedly their origin in the double capability of change of the hydro-circuits, and will therefore cease when this circumstance is taken into consideration on a repetition of the experiments.

The remarkable variability in the circle of action of one and the same multiplier in various circuits, and of different multipliers in the same circuit, is completely explained by the

* See Schweigger's *Jahrbuch*, 1826, Part 2, where I have given a somewhat more detailed explanation of the separate points.

preceding consideration. For if we denote by A the sum of the tensions, and by L the reduced length of any galvanic circuit,

$$\frac{A}{L}$$

expresses the magnitude of its current. If we now imagine a multiplier of n similar convolutions each of the reduced length λ,

$$\frac{A}{L + n\lambda}$$

indicates the magnitude of the current when the multiplier is brought into the circuit as an integral part. Moreover, if we grant, for the sake of simplicity, that each of the n convolutions exerts the same action on the magnetic needle, the action of the multiplier on the magnetic needle is evidently

$$\frac{nA}{L + n\lambda},$$

when the action of an exactly similar coil of the circuit, without the multiplier on the needle, is taken as

$$\frac{A}{L}.$$

Hence it follows directly that the action on the magnetic needle is augmented or weakened by the multiplier, according as nL *is greater or smaller than* L + $n\lambda$, *i. e., according as* n *times the reduced length of the circuit without the multiplier is greater or smaller than the reduced length of the circuit with the multiplier.* Further, a mere glance at the expression by which the action of the multiplier on the needle has been determined, will show that the greatest or smallest action occurs as soon as L may be neglected with reference to $n\lambda$, and is expressed by

$$\frac{A}{\lambda}.$$

If we compare this extreme action of the multiplier with that which a perfectly similarly constructed convolution of the circuit without the multiplier produces, we perceive that both are in the same ratio to one another as the reduced lengths L and λ, which relation may serve to determine one of the values when the others are known. *The expression found for the extreme action of the multiplier shows that it is proportional to the tension of the circuit, and independent of its reduced length*; conse-

quently the extreme action of the same multiplier may serve not merely to determine the tensions in various circuits, but it also indicates that the extreme action may be also augmented to the same degree as the sum of the tensions is increased, which may be effected by forming a voltaic combination with several simple circuits. If we represent the actual length of a coil of the multiplier by l, its conductibility by $\varkappa$, and its section by ω, so that $\lambda = \frac{l}{\varkappa \,.\, \omega}$, the expression for the extreme action of the multiplier is converted into

$$\varkappa \,.\, \omega \,.\, \frac{A}{l},$$

from which it will be seen *that the extreme action of two multipliers of different metals, constructed of wire of the same thickness, are in the same ratio to each other as the conductibilities of these metals, and that the extreme actions of two multipliers formed of wire of the same metal, are in the same proportion to each other as the sections of the wires.* All these various peculiarities of the multiplier I have shown to be founded on experience, partly on experiments performed by other persons, and partly on those by myself*. The most recent experiments made on this subject on thermo-circuits, have, in a different, and, in a certain sense, opposite manner, already afforded the conclusion deduced above from an equation of the reduced lengths, that the sum of the tensions in a thermo-circuit is far weaker than in the ordinary hydro-circuits; and a preliminary comparison has convinced me, that, with respect to the heating effects, if they are to be predicted with certainty, a voltaic combination of some hundred well-chosen simple thermo-circuits is requisite, and for chemical effects of some energy a far greater apparatus. Experiments, which place this prediction beyond doubt, will afford a new and not unimportant confirmation to the theory here propounded.

The previous considerations are also sufficient to indicate the process which is carried on when the galvanic circuit is divided at any place into two or more branches. For this purpose I call attention to the circumstance, that at the time we found the equation $S = \frac{A}{L}$, we also obtained the rule that the magnitude of the current in any homogeneous part of the galvanic

* Schweigger's *Jahrbuch*, 1826. Part 2; and 1827.

circuit is given by the quotient of the difference between the electrical forces existing at the extremities of the portion and its reduced length. It is true, this rule was only advanced above for the case in which the circuit nowhere divides into several branches; but a very simple consideration, analogous to the one then made, derived from the equality of the abducted and adducted quantity of electricity in all sections of each prismatic part, is sufficient to prove that the same rule holds good for every single branch in case of a division of the circuit. Let us suppose that the circuit be divided, for instance, into three branches, whose reduced lengths are λ, λ', λ''; and, moreover, that at each of these places the undivided circuit and the single branches possess equal electrical force, and consequently no tension occurs there, and designate by α the difference between the electrical forces at these two places; then, according to the above rule, the magnitude of the current in each of the three branches is

$$\frac{\alpha}{\lambda}, \quad \frac{\alpha}{\lambda'}, \quad \frac{\alpha}{\lambda''};$$

whence it directly follows *that the currents in the three branches are inversely as their reduced lengths*; so that each separate one may be found when the sum of all three together is known. But the sum of all three is evidently equal to the magnitude of the current at any other place of the non-divided portion of the circuit, for otherwise the permanent state of the circuit, which is still constantly supposed, would not be maintained. If we connect with this the conclusion resulting from the above considerations, namely, that the magnitude of the current, and the nature of each homogeneous part of the circuit, give the dip of the corresponding straight line, representing the separation of the electricity, we are certain that the figure of the separation belonging to the non-divided portion of the circuit must remain the same so long as the current in it retains the same magnitude, and *vice versâ*; whence it follows that the variability of the current in the non-divided portion necessarily supposes that the difference between the electrical forces at the extremities of this portion is constant. If we now imagine, instead of the separate branches, a single conductor of the reduced length Λ brought into the circuit which does not at all alter the magnitude of its current and its tensions, then, according to what has just been stated, the difference between the electrical forces

at its extremities must still always remain α, and consequently be

$$\frac{\alpha}{\Lambda} = \frac{\alpha}{\lambda} + \frac{\alpha}{\lambda'} + \frac{\alpha}{\lambda''},$$

or

$$\frac{1}{\Lambda} = \frac{1}{\lambda} + \frac{1}{\lambda'} + \frac{1}{\lambda''},$$

which equation serves to determine the value of Λ. But if this value is known, and we call A the sum of all the tensions in the circuit, and L the reduced length of the non-divided portion of the circuit, *we obtain, as is known, for the magnitude of the current in the last-mentioned circuit*

$$\frac{A}{L + \Lambda},$$

which is equal to the sum of the currents occurring in the separate branches. Now since it has already been proved that the currents in the separate branches are in inverse proportion to one another as the reduced lengths of these branches, we obtain for the magnitude of the current in the branch whose reduced length is λ,

$$\frac{A}{L + \Lambda} \cdot \frac{\Lambda}{\lambda};$$

in the branch whose reduced length is λ',

$$\frac{A}{L + \Lambda} \cdot \frac{\Lambda}{\lambda'};$$

and in the branch whose reduced length is λ'',

$$\frac{A}{L + \Lambda} \cdot \frac{\Lambda}{\lambda''}.$$

This remote, and hitherto but slightly noticed peculiarity of the galvanic circuit, I have also found to be perfectly confirmed by experiment*.

I herewith conclude the consideration of such galvanic circuits which have already attained the permanent state, and which neither suffer modifications by the influence of the surrounding atmosphere, nor by a gradual change in their chemical composition. But from this point the simplicity of the subject decreases more and more, so that the previous elementary treatment soon entirely disappears. With respect to those

* Schweigger's *Jahrbuch*, 1827.

circuits on which the atmosphere exercises some influence, and whose condition varies with time, without this change originating in a progressive chemical transformation of the circuit, and is thus distinguished from all the others by the magnitude of its current being different at different places,—I have been content, with respect to each of these, always to treat of only the most simple case, as they but rarely occur in nature, and in general appear to be of less interest. I have adopted this plan the more willingly, as I intend to return to this subject at some future time. But with regard to that modification of galvanic circuits which is produced by a chemical change in the circuit, proceeding first from the current, and then again reacting on it, I have devoted separate attention to it in the Appendix. The course adopted is founded on a vast number of experiments performed on this subject, the communication of which, however, I omit, because they appear to be capable of being far more accurately determined than I was able to do at that time, from failing to attend to several elements in operation; nevertheless, I consider it proper to mention the circumstance in this place, in order that the careful manner with which I advance in the inquiry, and which I consider to be due to truth, may not operate more than is just against its reception.

I have sought for the source of the chemical changes caused by the current, in the above-described peculiar mode of separation of the electricity of the circuit; and, I can scarcely doubt, have at least found the main cause. It is immediately evident that each disk belonging to a section of a galvanic circuit, which obeys the electric attractions and repulsions and does not oppose their movement, must in the closed circuit be propelled always towards one side only, as these attractions and repulsions, in consequence of the continually varying electric force, are different at the two sides; and it is mathematically demonstrable *that the force with which it is driven to the one side, is in the ratio compounded of the magnitude of the electric current and of the electric force in the disk.* It is true, however, that merely a change of position in space would be immediately produced by this. But if this disk be regarded as a compound body, the constituent parts of which, according to electro-chemical views, are distinguished by a difference in their electrical relation to one another, it thence directly follows that this

one-sided pressure on the various constituent parts would in most cases act with unequal force, and sometimes even in contrary direction, and must thus excite a tendency in them to separate from one another. From this consideration results a distinct activity of the circuit, tending to produce a chemical change in its parts, which I have termed its *decomposing force*, and I have endeavoured to determine its magnitude for each particular case. This determination is independent of the mode, in which the electricity may be conceived to be asssociated with the atoms.* Granting, which seems to be most natural, that the electricity is diffused proportionately to the mass over the space which these bodies then occupy, a complete analysis will show *that the decomposing force of the circuit is in direct proportion to the energy of the current, and, moreover, that it depends on a coefficient, to be derived from the nature of the constituent parts and their chemical equivalents.* From the nature of this decomposing force of the circuit, which is of equal energy at all places of an homogeneous portion, it directly follows, that when it is capable of overcoming, under all circumstances, the reciprocal connexion of the constituent parts, the separation and abduction of the constituents to both sides of the circuit are limited solely by mechanical obstacles; but if the connexion of the constituent parts *inter se*, either immediately at the commencement everywhere, or in the course of the action anywhere, overcome the decomposing force of the circuit, then from that time no further movement of the elements can take place. This general description of the decomposing force is in accordance with the experiments of Davy and others.

There is a peculiar state which seems to be produced in most cases of the separation of the two elements of a chemically compound liquid, which is especially worthy of attention, and which is caused in the following manner. When the decomposition is confined solely to a limited portion of the circuit, and the constituent parts of the one kind are propelled towards the one side of this part, and the constituent parts of the other kind to its opposite side, then, for this very reason, a natural limit is prescribed to the action; for the constituent

* I shall shortly have occasion to speak of the peculiar import of this remark, when I shall attempt to reduce the actions of the parts of a galvanic circuit on one another, as discovered by Ampère, to the usual electrical attractions and repulsions.

part preponderating on the one side of any disk, within the portion in the act of decomposition, will, by force of its innate repulsive power, constantly oppose the movement of a similar constituent to the same side, so that the decomposing force of the circuit has not merely to overcome the constant connexion of the two constituents *inter se*, but also this reaction of each constituent on itself. It is hence evident that a cessation in the chemical change must occur, if at any time there arises an equilibrium between the two forces. This state, founded on a peculiar chemical and permanent separation of the constituents of the portion of the circuit in the act of decomposition, is the very one from which I started, and whose nature I have endeavoured to determine as accurately as possible in the Appendix. Even the mere description of the mode of origin of this highly remarkable phænomenon shows that at the extremities of the divided portion no natural equilibrium can occur, on which account the two constituents must be retained at these two places by a mechanical force, unless they pass over to the next parts of the circuit, or, where the other circumstances allow, separate entirely from the circuit. Who would not recognise in this plain statement all the chief circumstances hitherto observed of the external phænomenon in chemical decompositions by the circuit?

If the current, and, at the same time, the decomposing force, be suddenly interrupted, the separated constituents gradually return to their natural equilibrium; but tend to re-assume immediately the relinquished state, if the current is re-established. During this process, both the conductibility, and the mode of excitation between the elements of the portion in the act of decomposition, obviously vary with their chemical nature; but this necessarily produces a constant change in the electrical separation, and in the magnitudes of the current in the galvanic circuit dependent thereon, which only finds its natural limits in the permanent state of the electrical separation. For the accurate determination of this last stage of the electric current it is requisite to be acquainted with the law which governs the conductibility and force of excitation of the variable mixtures, formed of two different liquids. Experiment has hitherto afforded insufficient data for this purpose, I have therefore given the preference to a theoretical supposition, which will supply its place until the true law is discovered.

With the help of this law, which is not altogether imaginary, I now arrive at the equations which make known for each case all the individual circumstances constituting the permanent state of the chemical separation in the galvanic circuit; I have, however, neglected the further use of them, as the present state of our experimental knowledge in this respect did not appear to me to repay the requisite trouble. Nevertheless, in order to compare in their general features the results of this examination with what has hitherto been supplied by experiments, I have fully carried out one particular case, and have found that the formula represents very satisfactorily the kind of wave of the force, as I have above described it*.

Having thus given a slight outline of the contents of this Memoir, I will now proceed to the fundamental investigation of the individual points.

* Schweigger's *Jahrbuch*, 1826. Part 2.

[To be continued.]

SCIENTIFIC MEMOIRS.

VOL. II.—PART VIII.

ARTICLE XIII. continued.

The Galvanic Circuit investigated Mathematically. *By* Dr. G. S. OHM*.

THE GALVANIC CIRCUIT.

A. *General observations on the diffusion of electricity.*

1. A PROPERTY of bodies, called into activity under certain circumstances, and which we call *electricity*, manifests itself in space, by the bodies which possess it, and which on that account are termed *electric*, either attracting or repelling one another.

In order to investigate the changes which occur in the electric condition of a body A in a perfectly definite manner, this body is each time brought, under similar circumstances, into contact with a second moveable body of invariable electrical condition, called the *Electroscope*, and the force with which the electroscope is repelled or attracted by the body is determined. This force is termed the *electroscopic* force of the body A; and to distinguish whether it is attractive or repulsive we place before the expression for its measure the sign + in the one case, and − in the other.

The same body A may also serve to determine the electroscopic force in various parts of the same body. For this purpose we take the body A of very small dimensions, so that when we bring it into contact with the part to be tested of any third body, it may from its smallness be regarded as a sub-

* "*Die Galvanische Kette mathematisch bearbeitet von Dr. G. S. Ohm*: *Berlin*, 1827."

stitute for this part; then its electroscopic force, measured in the way described, will, when it happens to be different at the various places, make known the relative difference with regard to electricity between these places.

The intention of the preceding explanations is to give a simple and determinate signification to the expression "electroscopic force"; it does not come within the limits of our plan to take notice either of the greater or less practicability of this process, nor to compare *inter se* the various possible modes of proceeding for the determination of the electroscopic force.

2. We perceive that the electroscopic force moves from one place to another, and from one body to another, so that it does not merely vary at different places at the same time, but also at a single place at different times. In order to determine in what manner the electroscopic force is dependent upon the time when it is perceived, and on the place where it is elicited, we must set out from the fundamental laws to which the exchange of electroscopic force occurring between the elements of a body is subject.

These fundamental laws are of two kinds, either borrowed from experiment, or, where this is wanting, assumed hypothetically. The admissibility of the former is beyond all doubt, and the justness of the latter is distinctly evident from the coincidence of the results deduced from calculation with those which actually occur; for since the phænomenon with all its modifications is expressed in the most determinate manner by calculation, it follows, since no new uncertainties arise and increase the earlier ones during the process, that an equally perfect observation of nature must in a decisive manner either confirm or refute its statements. This in fact is the chief merit of mathematical analysis, that it calls forth, by its never-vacillating expressions, a generality of ideas, which continually excites to renewed experiments, and thus leads to a more profound knowledge of nature. Every theory of a class of natural phænomena founded upon facts, which will not admit of analytical investigation in the form of its exposition, is imperfect; and no reliance is to be placed upon a theory developed in ever so strict a form, which is not confirmed to a sufficient extent by observation. So long therefore as not even one portion of the effects of a natural force has been observed with the greatest accuracy in all its gradations, the calculation employed in its investigation only treads

on uncertain ground, as there is no touchstone for its hypotheses, and in fact it would be far better to wait a more fit time; but when it goes to work with the proper authority, it enriches, at least in an indirect manner, the field it occupies with new natural phænomena, as universal experience shows. I have thought it necessary to premise these general remarks, as they not only serve to throw more light on what follows, but also because they explain the reason why the galvanic phænomena have not long since been mathematically treated with greater success, although, as we shall subsequently find, the requisite course has been already earlier pursued in another, apparently less prepared, branch of Physics.

After these reflections we will now proceed to the establishment of the fundamental laws themselves.

3. When two electrical elements, E and E′, of equal magnitude, of like form and similarly placed with respect to each other, but unequally powerful, are situated at the proper distance from each other, they exhibit a mutual tendency to attain electric equilibrium, which is apparent in both constantly and uninterruptedly approaching nearer to the mean of their electric state, until they have actually attained it. That is to say, both elements reciprocally change their electric state so long as a difference continues to exist between their electroscopic forces; but this change ceases as soon as they have both attained the same electroscopic force. Consequently this change of the electric difference of the elements is so dependent that the one disappears at the same time with the other. We now suppose that the change, effected in an extremely short instant of time in both elements, is proportional to the difference of their cotemporaneous electroscopic force and the magnitude of the instant of time; and without yet attending to any material distinctions of the electricity, it is always to be understood that the forces designated by + and − are to be treated exactly as opposite magnitudes. That the change is effected accurately according to the difference of the forces, is a mathematical supposition, the most natural because it is the most simple; all the rest is given by experiment. The motion of electricity is effected in most bodies so rapidly that we are seldom able to determine its changes at the various places, and on that account we are not in a condition to discover by observation the law according to which they act. The galvanic phænomena, in which such

changes occur in a constant form, are therefore of the highest importance for testing this assumption: for if the conclusions drawn from the supposition are thoroughly confirmed by those phænomena, it is admissible, and may then be applied without any further consideration to all analogous researches, at least within the same limits of force.

We have assumed, in accordance with the observations hitherto made, that when by any two exteriorly like constituted elements, whether they be of the same or of different matter, a mutual change in their electrical state is produced, the one loses just so much force as the other gains. Should it hereafter be shown by experiments that bodies exhibit a relation similar to that which in the theory of heat is termed the capacity of bodies, the law we have established will have to undergo a slight alteration, which we shall point out in the proper place.

4. When the two elements E and E′ are not of equal magnitude, it is still allowed to regard them as sums of equal parts. Granting that an element E consist of m perfectly equal parts, and the other E′ of m' exactly similar parts, then, if we imagine the elements E and E′ exceedingly small in comparison with their mutual distance, so that the distances from each part of the one to each part of the other element are equal, the sum of the actions of all the m' parts of the element E′ on a part of E will be m' times that which a single part exerts, and the sum of all the actions of the element E′ on all the m parts of E will be $m\,m'$ times that which a part of E′ exerts on a part of E. It is hence evident, that in order to ascertain the mutual actions of dissimilar elements on each other, they must be taken as proportional not merely to the difference of their electroscopic forces and their duration, but also to the product of their relative magnitudes. We shall in future term the sum of the electroscopic actions, referred to the magnitude of the elements—by which therefore we have to understand the force multiplied by the magnitude of the space over which it is diffused, in the case where the same force prevails at all places in this space—the *quantity of electricity*, without intending to determine anything thereby with respect to the material nature of electricity. The same observation is applicable to all figurative expressions introduced, without which, perhaps for good reasons, our language could not exist.

In cases where the elements cannot be regarded as evanescent

in comparison with their relative distances, a function, to be determined separately for each given case from their dimensions and their mean distance, must be substituted for the product of the magnitudes of the two elements, and which we will designate where it is employed by F.

5. Hitherto we have taken no notice of the influence of the mutual distance of the elements between which an equalization of their electric state takes place, because as yet we have only considered such elements as always retained the same relative distance. But now the question arises, whether this exchange is directly effected only between adjacent elements, or if it extends to others more distant, and how on the one or the other supposition is its magnitude modified by the distance? Following the example of Laplace, it is customary in cases where molecular actions at the least distance come into play, to employ a particular mode of representation, according to which a direct mutual action between two elements separated by others, still occurs at finite distances, which action, however, decreases so rapidly, that even at any perceptible distance, be it ever so minute, it has to be considered as perfectly evanescent. Laplace was led to this hypothesis, because the supposition that the direct action only extended to the next element produced equations, the individual members of which were not of the same dimension relatively to the differentials of the variable quantities*,—a non-uniformity which is opposed to the spirit of the differential calculus. This apparent unavoidable

* Poisson, in his Mémoire sur la Distribution de la Chaleur, *Journ. de l'Ecole Polytechn.* cah. xix. expresses himself on this subject thus:—

"If a bar be divided, by sections perpendicular to the axis, into an infinite number of infinitely small elements, and if we consider the mutual action of three consecutive elements, that is to say, the quantity of heat that the intermediate element at each instant communicates to and abstracts from the two others, in proportion to the positive or negative excess of its temperature over that of each of them, we may thence easily determine the augmentation of temperature of this element during an infinitely small instant; assuming therefore this quantity equal to the differential of its temperature taken with respect to the time, the equation of the propagation of heat according to the length of the bar is formed; but on examining the question more attentively, it is seen without difficulty that this equation would be founded on the comparison of two infinitely small non-homogeneous quantities, or of different orders, which would be contrary to the first principles of the differential calculus. This difficulty can only be made to disappear by supposing, as M. Laplace first remarked, (Mémoires de la 1re classe de l'Institut, année 1809,) that the action of each element of the bar extends itself beyond the contact, and that it exerts itself on all the elements contained within a finite space, as small as we please."

disproportion between the members of a differential equation, belonging nevertheless necessarily to one another, is too remarkable not to attract the attention of those to whom such inquiries are of any value; an attempt therefore to add something to the explanation of this ænigma will be the more proper in this place, as we gain the advantage of rendering thereby the subsequent considerations more simple and concise. We shall merely take as an instance the propagation of electricity, and it will not be difficult to transfer the obtained results to any other similar subject, as we shall subsequently have occasion to demonstrate in another example.

6. Above all, it is requisite that the term goodness of conduction be accurately defined. But we express the energy of conduction between two places by a magnitude which, under otherwise similar circumstances, is proportional to the quantity carried over in a certain time from one place to the other multiplied by the distance of the two places from each other. If the two places are extended, then we have to understand by their distance the straight line connecting the centres of the dimensions of the two places. If we transfer this idea to two electric elements, E and E′, and call s the mutual distance of their centres, ϱ the quantity of electricity, which under accurately determined and invariable circumstances is carried over from one element to the other, and $\varkappa$ the conductibility between them,

$$\varkappa = \varrho \, . \, s.$$

We will now endeavour to determine more precisely the quantity of electricity denoted by ϱ. According to § 4 the quantity of electricity, which is transferred in an exceedingly short time from one element to the other, is, the distance being invariable, in general proportional to the difference between the electroscopic forces, the duration, and the size of each of the two elements. If therefore we designate the electroscopic forces of the two elements E and E′ by u and u', and the space they occupy by m and m', we obtain for the quantity of electricity carried over from E′ to E in the element of time $d\,t$ the following expression:

$$\alpha\, m\, m' \,(u' - u)\, d\,t,$$

where α represents a coefficient depending in some way on the distance s. This quantity changes every moment if $u' - u$ is variable; but if we suppose that the forces u' and u remain

constant at all times, it merely depends on the magnitude of the instant of time $d\,t$, we can consequently extend it to the unity of time; if we place the present constant difference of the forces $u' - u$ equal to the unity of force, it then becomes

$$\alpha\, m\, m'.$$

This quantity of electricity is for the two elements E and E′ whose position is invariable, constant under the same circumstances, on which account it may be employed in the determination of the power of conduction just mentioned. For if we understand by ϱ the quantity of electricity transferred from E′ to E in the unity of time, with a constant difference of the electroscopic forces equal to the unity of force, we have

$$\varrho = \alpha\, m\, m',$$

and then

$$\varkappa = \alpha\, m\, m'\, s.$$

If we take from this last equation the value of $\alpha\, m\, m'$ and substitute it in the expression

$$\alpha\, m\, m'\, (u' - u)\, d\,t,$$

we obtain for the variable quantity of electricity which passes over in the instant of time $d\,t$ from E′ to E, the following:

$$\frac{\varkappa\, (u' - u)\, d\,t}{s}, \qquad (\delta)$$

which expression is not accompanied by the above-mentioned disproportion between the members of the differential equation, as will soon be perceived.

7. The course hitherto pursued was based upon the supposition that the action exerted by one element on the other is proportional to the product of the space occupied by the two elements, an assumption which, as was already observed in § 4, can no longer be allowed in cases where it is a question of the mutual action of elements situated indefinitely near each other, because it either establishes a relation between the magnitudes of the elements and their mutual distances, or prescribes to these elements a certain form. The previously found expression (δ) for the variable quantity of electricity passing from one element to the other, possesses therefore no slight advantage in being entirely independent of this supposition; for whatever may have to be placed in any determinate case instead of the product $m\, m'$, the expression (δ) constantly remains the

same, this peculiarity being solely referrible to the power of conduction $\varkappa$. If, for instance, F designate, as was stated in § 4, the function, corresponding to such a case, of the dimensions and of the mean distance of both elements, the expression

$$\alpha\, m\, m'\, (u' - u)\, d\, t$$

not merely changes apparently into

$$\mathrm{F}\, (u' - u)\, d\, t,$$

but also the equation

$$\varkappa = \alpha\, m\, m'\, s$$

into the other,

$$\varkappa = \mathrm{F} \,.\, s, \qquad (\odot)$$

so that if we take the value of F from this equation and place it in the above expression, we always obtain

$$\frac{\varkappa\, (u' - u)\, d\, t}{s}.$$

Moreover, the circumstance of the expression (♂) still remaining valid for corpuscles, whose dimensions are no longer indefinitely small, is of some importance when the same electroscopic force only exists merely at all points of each such part. It is hence evident how intimately our considerations are allied to the spirit of the differential calculus; for uniformity in all points with reference to the property which enters into the calculation is precisely the distinctive characteristic required by the differential calculus from that which it is to receive as an element.

If we institute a more profound comparison between the process originating with Laplace and that here advanced, we shall arrive at some interesting points of comparison. If for instance we consider that for infinitely small masses at infinitely short distances all particular relations must necessarily have the same weight as for finite masses at finite distances, it is not directly evident how the method of the immortal Laplace—to whom we are indebted for so many valuable explanations respecting the nature of molecular actions,—according to which the elements must be constantly treated as if they were placed at finite distances from each other, could nevertheless still afford correct results; but we shall find on closer examination that it acts in fact otherwise than it expresses. Indeed, since Laplace, when determining the changes of an element by all surrounding it, makes the higher powers of the distance disappear compared with the lower, he therewith assumes, quite in the spirit of the

differential calculus, the difference of action itself to be infinitely small, but terms it finite, and treats it also as such; whence it is immediately apparent that he in fact treats that which is infinitely small at an infinitely short distance as finite. Disregarding however the great certainty and distinctness which accompany our manner of representation, there might still be something more to say, and perhaps with some justice, against Laplace's mode of treatment in favour of ours, in this respect, that the former takes not the least account of the possible nature of the *given* elements of bodies, but merely has to do with *imaginary* elements of space, by which the physical nature of the bodies is almost entirely lost sight of. We may, to render our assertion intelligible by an example, undoubtedly imagine bodies in nature which consist only of homogeneous elements, but whose position to each other, taken in one direction, might be different than when in another direction; such bodies, as our mode of representation immediately shows, might conduct the electricity in one direction in a different manner than in another, notwithstanding that they might appear uniform and equally dense. In such a case, did it occur, we should have to take refuge, according to Laplace, in considerations which have remained entirely foreign to the general process. On the other hand, the mode in which bodies conduct affords us the means by which we are enabled to judge of their internal structure, which, from our almost total ignorance on the subject, cannot be immediately shown. Lastly, we may add, that this, our hitherto developed view of molecular actions, unites in itself the two advanced by Laplace and by Fourier in his theory of heat, and reconciles them with each other.

8. We need now no longer hesitate about allowing the electrical action of an element not to extend beyond the adjacent surrounding elements, so that the action entirely disappears at every finite distance, however small. The extremely limited circle of action with the almost infinite velocity with which electricity passes through many bodies might indeed appear suspicious; but we did not overlook on its admission, that our comparison in such cases is only effected by an imaginary relative standard, which is deceitful, and does therefore not justify us to vary a law so simple and independent until the conclusions drawn from it are in contradiction to nature, which in our subject, however, does not seem to be the case.

The sphere of action thus fixed by us, has, although it is infinitely small, precisely the same circumference as that introduced by Laplace, and called finite, where he lets the higher powers of the distance vanish compared with the lower, the reason of which may be found already in what has been stated above. The supposition of a finite distance of action in our sense would correspond to the case where Laplace still retains higher powers of distance together with the lower.

9. The bodies on which we observe electric phænomena are in most cases surrounded by the atmosphere; it is therefore requisite, in order to investigate profoundly the entire process, not to disregard the changes which may be produced by the adjacent air. According to the experiments left us by Coulomb on the diffusion of electricity in the surrounding atmosphere, the loss in force thus occasioned is (during a very short constant time), at least when the intensities are not very considerable, on the one hand proportional to the energy of the electricity, and on the other is dependent on a coefficient varying according to the cotemporaneous nature of the air, but otherwise invariable for the same air. The knowledge of this enables us to bring the influence of the atmosphere on galvanic phænomena into calculation wherever it might be requisite. It must however not be overlooked here, that Coulomb's experiments were made on electricity which had entered into equilibrium and was no longer in the process of excitation, with respect to which both observation and calculation have convinced us that it is confined to the surface of bodies, or merely penetrates to a very slight depth into their interior; for from thence may be drawn the conclusion, of some importance with respect to our subject, that all the electricity present in those experiments may have been directly concerned in the transference to the atmosphere. If we now connect with this observation the law just announced, according to which two elements, situated at any finite distance from each other, no longer exert any direct action on each other, we are justified in concluding, that where the electricity is uniformly diffused throughout the entire mass of a finite body, or at least so that proportionately but a small quantity resides in the vicinity of the surface, which case does not in general occur when it has entered into motion, the loss which is occasioned by the circumambient air can be but extremely small in comparison to that which takes place when the

entire force is situated immediately at the surface, which invariably happens when it has entered into equilibrium; and thence, therefore, it happens that the atmosphere exerts no perceptible influence on galvanic phænomena in the closed circuit when this is composed of good conductors, so that the changes produced by the presence of the atmosphere in phænomena of contact-electricity may be neglected in such cases. This conclusion, moreover, receives new support from the circumstance, that in the same cases the contact-electricity only remains during an exceedingly short time in the conductors, and even on that account would only give up a very slight portion to the air, even if it were in immediate contact with it.

Although, from what has been stated, it is placed beyond all doubt that the action of the atmosphere has no perceptible influence on the magnitude of effect of the usual galvanic circuits, it by no means is intended to admit the reverse of the conclusion, viz. that the galvanic conductor exerts no perceptible influence on the electric state of the atmosphere; for mathematical investigation teaches us that the electroscopic action of a body on another has no direct connexion with the quantity of electricity which is carried over from the one to the other.

10. We arrive at last at that position founded on experiment, and which is of the highest importance for the whole of natural philosophy, since it forms the basis of all the phænomena to which we apply the name of galvanic: it may be expressed thus: Different bodies, which touch each other, constantly preserve at the place of contact the same difference between their electroscopic forces by virtue of a contrariety proceeding from their nature, which we are accustomed to designate by the expression *electric tension*, or *difference of bodies*. Thus enounced, the position stands, without losing any of its simplicity, in all the generality which belongs to it; for we are nearly always referred to it by every single phænomenon. Moreover, the above expression is adopted in all its generality, either expressly or tacitly, by all philosophers in the explanation of the electroscopic phænomena of the voltaic pile. According to our previously developed ideas respecting the mode in which elements act on one another, we must seek for the source of this phænomenon in the elements directly in contact, and consequently we must allow the abrupt transition to take place from one body to the other in an infinitely small extent of space.

11. This being established, we will now proceed to the subject, and in the first place consider the motion of the electricity in a homogeneous cylindric or prismatic body, in which all points throughout the whole extent of each section, perpendicular to its axis, possess contemporaneously equal electroscopic force, so that the motion of the electricity can only take place in the direction of its axis. If we imagine this body divided by a number of such sections into disks of infinitely small thickness, and so that in the whole circumference of each disk the electroscopic force does not vary sensibly for each pair of such disks, the expression (δ) given in § 6 can be applied to determine the quantity of electricity passing from one disk to the other; but by the limitation of the distance of action to only infinitely small distances mentioned in the preceding paragraph, its nature is so modified that it disappears as soon as the divisor ceases to be infinitely small.

If we now choose one of the infinite number of sections invariably for the origin of the abscissæ, and imagine anywhere a second, whose distance from the first we denote by x, then $d\,x$ represents the thickness of the disk there situated, which we will designate by M. If we conceive this thickness of the disk to be of like magnitude at all places, and term u the electroscopic force present at the time t in the disk M, whose abscissa is x, so that therefore u in general will be a function of t and x; if we further suppose u' and $u_{\prime}$ to be the values of u when $x + d\,x$ and $x - d\,x$ are substituted respectively for x, then u' and $u_{\prime}$ evidently express the electroscopic forces of the disks situated next the two sides of the disk M, of which we will denote the one belonging to the abscissa $x + d\,x$ by M$'$, and that belonging to the abscissa $x + d\,x$ by M$_{\prime}$; and it is clearly evident that the distance of the centre of each of the disks M$'$ and M$_{\prime}$ from the centre of the disk M is $d\,x$. Consequently, by virtue of the expression (δ) given in § 6, if $\varkappa$ represents the conducting power of the disk M$'$ to M,

$$\frac{\varkappa\,(u' - u)\,d\,t}{d\,x}$$

is the quantity of electricity which is transferred during the interval of time $d\,t$ from the disk M$'$ to the disk M, or from the latter to the former, according as $u' - u$ is positive or negative. In the same manner, when we admit the same power of conduction between M$_{\prime}$ and M,

$$\frac{\varkappa\,(u_{\prime}-u)\;d\,t}{d\,x}$$

is the quantity of electricity passing over from $M_{\prime}$ to M when the expression is positive, and from M to $M_{\prime}$ when it is negative. The total change of the quantity of electricity which the disk M undergoes from the motion of the electricity in the interior of the body in the particle of time $d\,t$, is consequently

$$\frac{\varkappa\,(u' + u_{\prime} - 2\,u)\;d\,t}{d\,x},$$

and an increase in the quantity of electricity is denoted when this value is positive, and when negative a diminution of the same.

But according to Taylor's theorem

$$u' = u + \frac{d\,u}{d\,x}\,.\,d\,x + \frac{d^2\,u}{d\,x^2}\,.\,\frac{d\,x^2}{2} + \ldots\ldots,$$

and in the same way

$$u_{\prime} = u - \frac{d\,u}{d\,x}\,.\,d\,x + \frac{d^2\,u}{d\,x^2}\,.\,\frac{d\,x^2}{2} - \ldots\ldots;$$

consequently

$$u' + u_{\prime} = 2\,u + \frac{d^2\,u}{d\,x^2}\,d\,x^2.$$

According to this the expression just found for the total change of the quantity of electricity present in the disk M is converted during the time $d\,t$ into

$$\varkappa\,.\,\frac{d^2\,u}{d\,x^2}\,d\,x\;\,d\,t,$$

where $\varkappa$ represents the power of conduction which prevails from one disk to the adjacent one, which we suppose to be invariable throughout the length of the homogeneous body. It must here be observed, that this value $\varkappa$ is, on account of the infinitely small distance of action, proportional to the section of the cylindric or prismatic body; if therefore we denote the magnitude of this section by ω, and separate this factor from the value $\varkappa$, always calling the remaining portion $\varkappa$, the former expression changes into the following:

$$\varkappa\,\omega\,\frac{d^2\,u}{d\,x^2}\,d\,x\;\,d\,t,$$

in which $\varkappa$ now represents the conductibility of the body independent of the magnitude of the section, which we will term the *absolute* conductibility of the body in opposition to the former, which may be called the *relative*. Henceforward wherever

the word conductibility occurs without any closer definition, the absolute conductibility is always to be understood.

Hitherto we have left out of consideration the change which the disk suffers from the adjacent atmosphere. This influence may easily be determined. If, for instance, we designate by c the circumference of the disk belonging to the abscissa x, then $c\,d\,x$ is the portion of its surface which is exposed to the air; consequently, according to the experiments of Coulomb, mentioned in § 9,

$$b\,c\,u\,d\,x\,d\,t$$

is the change of the quantity of electricity which is occasioned in the disk M by the passing off of the electricity into the atmosphere during the moment of time $d\,t$, where b represents a coefficient dependent on the cotemporaneous nature of the atmosphere, but constant for the same atmosphere. It expresses a decrease when u is positive, and an increase when u is negative. But in accordance with our original supposition, this action cannot occasion an inequality of the electroscopic force in the same section of the body; or at least, this inequality must be so slight that no perceptible alteration is produced in the other quantities; a circumstance which may nearly always be supposed in the galvanic circuit.

Accordingly, the entire change which the quantity of electricity in the disk M undergoes in the moment of time $d\,t$ is

$$\varkappa\,\omega\,\frac{d^2u}{dx^2}\,d\,x\,.\,dt - b\,c\,u\,d\,x\,d\,t,$$

in which the portion is comprised which arises from the motion of the electricity in the interior of the body as well as that which is caused by the circumambient atmosphere.

But the entire change of the electroscopic force u in the disk M effected in the moment of time dt is

$$\frac{du}{dt}\,dt,$$

consequently the total change in the quantity of electricity in the disk M during the time dt is

$$\omega\,\frac{du}{dt}\,dx\ dt,$$

where, however, it is supposed that under all circumstances similar changes in the electroscopic force correspond to similar changes in the quantity of electricity. If observation showed that different bodies of the same surface underwent a different

change in their electroscopic force by the same quantity of electricity, then there would still remain to be added a coefficient γ corresponding to this property of the various bodies. Experience has not yet decided respecting this supposition borrowed from the relation of heat to bodies.

If we assume the two expressions just found for the entire change in the quantity of electricity in the disk M during the moment of time $d\,t$ to be equal, and divide all the members of the equation by $\omega\ dx\ dt$, we obtain

$$\gamma \frac{d\,u}{d\,t} = \varkappa \frac{d^2 u}{d\,x^2} - \frac{b\,c}{\omega}\,u, \qquad (a)$$

from which the electroscopic force u has to be determined as a function of x and t.

12. We have in the preceding paragraph found for the change in the quantity of electricity occurring between the disks M′ and M during the time $d\,t$

$$\frac{\varkappa\,(u'-u)\,d\,t}{d\,x},$$

and have seen that the direction of the passage is opposed to the course of the abscissæ when the expression is positive; on the contrary, it proceeds in the direction of the abscissæ when it is negative. In the same way the magnitude of the transition between the disks $M_{,}$ and M, when we retain the same relation to its direction, is

$$\frac{\varkappa\,(u_{,}-u)\,d\,t}{d\,x}.$$

If we substitute in these two expressions for $u_{,}$ and u' the transformations given in the same paragraph, and at the same time $\varkappa\,\omega$ for $\varkappa$, *i. e.* the absolute power of conduction for the relative, we obtain in both cases

$$\varkappa\,\omega \frac{d\,u}{d\,x}\,d\,t,$$

whence it results that the same quantity of electricity which enters from the one side into the disk M during the element of time $d\,t$, is again in the same time expelled from it towards the other side. If we imagine this transmission of the electricity, occurring at the time t in the disk belonging to the abscissa x, of invariable energy reduced to the unity of time, call it the *electric current*, and designate the magnitude of this current by S, then

$$S = \varkappa \, \omega \frac{du}{dx}; \qquad (b)$$

and in this equation positive values for S show that the current takes place opposed to the direction of the abscissæ; negative, that it occurs in the direction of the abscissæ.

13. In the two preceding paragraphs we have constantly had in view a homogeneous prismatic body, and have inquired into the diffusion of the electricity in such a body, on the supposition that throughout the whole extent of each section, perpendicular to its length or axis, the same electroscopic force exists at any time whatsoever. We will now take into consideration the case where two prismatic bodies A and B, of the same kind, but formed of different substances, are adjacent, and touch each other in a common surface. If we establish for both A and B the same origin of abscissæ, and designate the electroscopic force of A by u, that of B by u', then both u and u' are determined by the equation (a) in paragraph 11, if $\varkappa$ only retain the value each time corresponding to the peculiar substance of each body: but u represents a function of t and x, which holds only so long as the abscissa x corresponds to points in the body A; u on the other hand denotes a function of t and x, which holds only when the abscissa x corresponds to the body B. But there are still some other conditions at this common surface, which we will now explain. If we denote for this purpose the separate values of u and u', which they first assume at the common surface, by enclosing the general ones between crotchets, we find according to the law advanced in § 10 the following equation between these separate values:

$$(u) - (u') = a,$$

where a represents a constant magnitude otherwise dependent on the nature of the two bodies. Besides this condition, which relates to the electroscopic force, there is still a second, which has reference to the electric current. It consists in this, that the electric current in the common surface must in the first place possess equal magnitude and like direction in both bodies, or, if we retain the common factor ω,

$$\varkappa \, \omega \left(\frac{du}{dx}\right) = \varkappa' \, \omega \left(\frac{du'}{dx}\right),$$

where $\varkappa$ represents the actual power of conduction of the

body A, $\varkappa'$ that of the body B, and $\left(\frac{d\,u}{d\,x}\right), \left(\frac{d\,u'}{d\,x}\right)$ the particular values of $\frac{d\,u}{d\,x}, \frac{d\,u'}{d\,x}$ immediately belonging to them at the common surface, and in which it was assumed that the origin of the abscissæ was not taken on this common surface. The necessity of this last equation may easily be conceived; for were it otherwise, the two currents would not be of equal energy in the common surface, but there would be more conveyed from the one body to this surface than would be abstracted from it by the other; and if this difference were a finite portion of the entire current, the electroscopic force would increase at that very place, and indeed, considering the surprising fertility of the electric current, would arrive in the shortest time to an exceedingly high degree, as observation has long since demonstrated. Nor can a smaller quantity of electricity be imparted from the one body to the common surface than it is deprived of by the other, as this circumstance would be evinced by an infinitely high degree of negative electricity.

It is not absolutely requisite for the validity of the preceding determinations, that the two bodies in contact have the same base. The section in the one prismatic body may be different in size and form to that in the other, if this does not render the electroscopic force sensibly different at the various points of the same section, which, considering the great energy with which the electricity tends to equilibrium, will not be the case when the bodies are good conductors, whose length far surpasses their other dimensions. In this case everything remains as before, only that the section of the body B must everywhere be distinguished from that of A; consequently the second conditional equation for the place where the two bodies are in contact changes into the following:—

$$\varkappa\,\omega\,\left(\frac{d\,u}{d\,x}\right) = \varkappa'\,\omega'\,\left(\frac{d\,u'}{d\,x}\right),$$

where ω still represents the section of A, but ω' that of the body B, which at present differs from the former.

There may even exist in the prolongation of the body A two prismatic bodies, B and C, separated from each other, which are both situated immediately on the one surface of A. If in this case $\varkappa'\,\omega'\,u'$ signifies for the body B, and $\varkappa''\,\omega''$, u'' for the body

C what $\varkappa\,\omega\,u$ does for A, we obtain instead of the one conditional equation the two following:—

$$(u) - (u') = a,$$
$$(u) - (u'') = a',$$

where a represents the electric tension between the bodies A and B, and a' that between A and C. In the same manner we now obtain instead of the second conditional equation the following:—

$$\varkappa\,\omega\left(\frac{d\,u}{d\,x}\right) = \varkappa'\,\omega'\left(\frac{d\,u'}{d\,x}\right) + \varkappa''\,\omega''\left(\frac{d\,u''}{d\,x}\right).$$

It is immediately apparent how these equations must change when a greater number of bodies are combined. We shall not enter further into these complications, as what has been stated suffices to throw sufficient light upon the changes which have in such a case to be performed on the equations.

14. To avoid misconception, I will, at the close of these general observations, once more accurately define the circle of application within which our formulæ have universal validity. Our whole inquiry is confined to the case where all the parts of the same section possess equal electroscopic force, and the magnitude of the section varies only from one body to the other. The nature of the subject, however, frequently gives rise to circumstances which render one or the other of these conditions superfluous, or at least diminishes their importance. Since the knowledge of such circumstances is not without use, I will here illustrate the most prominent by an example.

A circuit of copper, zinc, and an aqueous fluid, will wholly come under the above formula when the copper and zinc are prismatic and of equal section; when, further, the fluid is likewise prismatic and of the same or of smaller section, and its terminal surfaces everywhere in contact with the metals. Nay, when only these last conditions are fulfilled with respect to the fluid, the metals may possess equal sections or not, and touch one another with their full sections, or only at some points, and even their form may deviate considerably from the prismatic form, and nevertheless the circuit must constantly obey the laws deduced from our formulæ; for the motion of the electricity produced with such ease in the metals, is obstructed to such a considerable extent by the non-conductive nature of the fluid, that it gains sufficient time to diffuse itself thoroughly

with equal energy over the metals, and thus re-establishes in the fluid the conditions upon which our calculation is founded. But it is a very different matter when the prismatic fluid is only touched in disproportionately small portions of its surfaces by the metals, as the electricity arriving there can only advance slowly and with considerable loss of energy to the untouched surfaces of the fluid, whence currents of various kinds and directions result. The existence of such currents has been sufficiently demonstrated by Pohl's manifoldly varied experiments, and nothing more now stands in the way of their determination by analysis, after the additions which it has received from the successful investigations respecting the theory of heat, than the complexities of the expressions. Since their determination exceeds the limits of this small work, which has for its object to investigate the current only in one dimension, we will defer them to a more fit occasion.

We will now proceed to the application of the formulæ advanced, and divide, for the sake of a more easy and general survey, the whole into two sections, of which the one will treat of the electroscopic phænomena, and the other of the phænomena of the electric current.

B. *Electroscopic Phænomena.*

15. In our preceding general determinations we have constantly confined our attention to prismatic bodies, whose axes, upon which the abscissæ have been taken, formed a straight line. But all these considerations still retain their entire value, if we imagine the conductor constantly curved in any way whatsoever, and take the abscissæ on the present curved axis of the conductor. The above formulæ acquire their entire applicability from this observation, since galvanic circuits, from their very nature, can but seldom be extended in a straight line. Having anticipated this point, we will immediately proceed to the most simple case, where the prismatic conductor is formed in its entire length of the same material, and is curved backwards on itself, and conceive the seat of the electric tension to be where its two ends touch. Although no case in nature resembles this imaginary one, it will nevertheless be of great service in the treatment of the other cases which do really occur in nature.

The electroscopic force, at any place of such a prismatic body, may be deduced from the differential equation (a) found in § 11. For this purpose we have only to integrate it, and to determine, in accordance with the other conditions of the problem, the arbitrary functions or constants entering into the integral. This matter is, however, generally very much facilitated, with respect to our subject, by omitting one or even two members, according to the nature of the subject, from the equation (a). Thus nearly all galvanic actions are such that the phænomena are permanent and invariable immediately at their origin. In this case, therefore, the electroscopic force is independent of time, consequently the equation (a) passes into

$$o = \varkappa \frac{d^2 u}{d x^2} - \frac{b c}{\omega} u.$$

Moreover, the surrounding atmosphere has (as we have already noticed in § 9.) in most cases no influence on the electric nature of the galvanic circuit; then $b = o$, by which the last equation is converted into

$$o = \frac{d^2 u}{d x^2}.$$

But the integral of this last equation is

$$u = f x + c, \qquad (c)$$

where f and c represent any constants remaining to be determined. The equation (c) consequently expresses the law of electrical diffusion, in a homogeneous prismatic conductor, in all cases where the abduction by the air is insensible, and the action no longer varies with time. As these circumstances in reality most frequently accompany the galvanic circuit, we shall on that account dwell longest upon them.

We are enabled to determine one of the constants by the tension occurring at the extremities of the conductor, which has to be regarded as invariable and given in each case. If, for instance, we imagine the origin of the abscissæ anywhere in the axis of the body, and designate the abscissa belonging to one of its ends by x_1, then the electroscopic force there situated is, according to the equation (c),

$$f x_1 + c;$$

in the same way we obtain for the electroscopic force of the other extremity, when we represent its abscissa by x_2,

$$f x_2 + c.$$

If we now call the given tension or difference of the electroscopic force a, we have

$$a = \pm f(x_1 - x_2).$$

But $x_1 - x_2$ evidently represents the entire, positive or negative, length of the prismatic conductor; if we designate this by l, we obtain accordingly

$$a = \pm f l,$$

whence the constant f may be determined. If we now introduce the value of the constant thus found into the equation (c), it is converted into

$$u = \pm \frac{a}{l} x + c,$$

so that only the constant c remains to be determined. We may consider the ambiguity of the sign $\pm$ to be owing to the tension a, by ascribing to it a positive value when the extremity of the conductor, belonging to the greater abscissa, possesses the greatest electroscopic force, and when the contrary a negative. Under this supposition is then generally

$$u = \frac{a}{l} x + c. \qquad (d)$$

The constant c remains in general wholly undetermined, which admits of our allowing the diffusion of the electricity in the conductor to vary arbitrarily, by external influences, in such manner that it occupies the entire conductor everywhere uniformly.

Among the various considerations respecting this constant, there is one of especial importance to the galvanic circuit, I mean that which supposes the circuit to be connected at some one place with a perfect conductor, so that the electroscopic force has to be regarded as constantly destroyed at this place. If we call the abscissa belonging to this place λ, then according to the equation (d)

$$o = \frac{a}{l} \lambda + c.$$

By determining from this the constant c, and placing its value in the same equation (d), we obtain

$$u = \frac{a}{l}(x - \lambda),$$

from which the electroscopic force of a galvanic circuit of the

length l, and of the tension a, which is touched at any given place whose abscissa is λ, may be found for every other place.

If any constant and perfect adduction, from outwards to the galvanic circuit, were to be given instead of the permanent abduction outwards, so that the electroscopic force pertaining to the abscissa λ were compelled to assume constantly a given energy, which we will designate by α, we should obtain for the determination of the constant c the equation

$$\alpha = \frac{a}{l}\lambda + c,$$

and for the determination of the electroscopic force of the circuit at any other place the following:

$$u = \frac{a}{l}(x - \lambda) + \alpha.$$

We have seen how the constant c may be determined when the electroscopic force is indicated at any place of the circuit by external circumstances; but now the question arises, what value are we to ascribe to the constant when the circuit is left entirely to itself, and this value can consequently no longer be deduced from outward circumstances? The answer to this question is found in the consideration, that each time both electricities proceed contemporaneously, and in like quantity from a previously indifferent state. It may, therefore, be asserted, that a simple circuit of the present kind, which is formed in a perfectly neutral and isolated condition, would assume on each side of the place of contact an equal but opposite electric condition, whence it is self-evident that their centre would be indifferent. For the same reason, however, it is also apparent that when the circuit at the moment of its origin is compelled by any circumstance to deviate from this, its normal state, it would certainly assume the abnormal one until again caused to change.

The properties of a simple galvanic circuit, such as we have hitherto considered them to be, accordingly consist essentially in the following, as is directly evident from the equation (d):

a. The electroscopic force of such a circuit varies throughout the whole length of the conductor continually, and on like extents constantly to the same amount; but where the two extremities are in contact, it changes suddenly, and, indeed,

from one extremity to the other, to the extent of an entire tension.

b. When any place of the circuit is disposed by any circumstance to change its electric state, all the other places of the circuit change theirs at the same time, and to the same amount.

16. We will now imagine a galvanic circuit, composed of two parts, P and P′, at whose two points of contact a different electric tension occurs, which case comprises in it the thermal circuit. If we call u the electroscopic force of the part P, and u' that of the part P′, then, according to the preceding paragraph, as here, the case there noticed is repeated twice, in consequence of the equation (c),

$$u = fx + c$$

for the part P, and

$$u' = f'x + c'$$

for the part P′, where f, c, f', c' are any constant magnitudes to be deduced from the peculiar circumstances of our problem, and each equation is only valid so long as the abscissæ refer to that part to which the equations belong. If we now place the origin of the abscissæ at one of the places of contact of the part P, and suppose the direction of the abscissæ in this part to proceed inwards; moreover, designate by l the length of the part P, and by l' that of P′; and, lastly, represent by u'_2 and u_1 the values of u and u' at the place of contact where $x = o$, and by u_2 and u'_1 the values of u and u' at the place of contact where $x = l$, we then obtain

$$u'_2 = f'(l + l') + c' \qquad u_1 = c$$
$$u_2 = fl + c \qquad u'_1 = f'l + c'.$$

If we now designate by a the tension which occurs at the place of contact where $x = o$, and by a' that which occurs at the place of contact where $x = l$; and if we once for all assume, for the sake of uniformity, that the tension at each individual place of contact always expresses the value which is obtained when we deduct the electroscopic force of one extremity from the electroscopic force of that extremity belonging to the place in question, upon which the abscissa falls before the abrupt change takes place—(it is not difficult to perceive that this general rule contains that advanced in the preceding paragraph, and which, in fact, expresses nothing more than that the tensions of such

places of contact, by the springing over of which, in the direction of the abscissæ, we arrive from the greater to the smaller electroscopic force, are to be regarded as positive, in the contrary case as negative, where, however, it must not be overlooked that every positive force has to be taken as greater than every negative, and the negative as greater than the actually smaller), we obtain

$$a = f' (l + l') + c' - c,$$

and

$$a' = f l - f' l + c - c',$$

whence directly results

$$a + a' = f l + f' l'.$$

But now at each of the places of contact when $\varkappa$ and ω represent the power of conduction and the section of the part P, and $\varkappa'$ and ω' the same for P′, in accordance with the considerations developed in § 13, there arises the conditional equation

$$\varkappa \, \omega \left(\frac{d\,u}{d\,x}\right) = \varkappa' \omega' . \left(\frac{d\,u'}{d\,x}\right),$$

where $\left(\frac{d\,u}{d\,x}\right)$ and $\left(\frac{d\,u'}{d\,x}\right)$ represent the values of $\frac{d\,u}{d\,x}$ and $\frac{d\,u'}{d\,x}$ at the place of contact. From the equations at the commencement of this paragraph for the determination of the electroscopic force in each single part of the circuit, we, however, obtain the value of x to be allowed to each,

$$\frac{d\,u}{d\,x} = f \text{ and } \frac{d\,u'}{d\,x} = f',$$

which converts the conditional equation in question into

$$\varkappa \, \omega f = \varkappa' \omega' f'.$$

From this, and the equation $a + a' = f l + f' l'$ just deduced from the tensions, we now find the values of f and f' thus:

$$f = \frac{(a + a') \, \varkappa' \omega'}{\varkappa' \omega' l + \varkappa \, \omega \, l'},$$

$$f' = \frac{(a + a') \, \varkappa \, \omega}{\varkappa' \omega' l + \varkappa \, \omega \, l'},$$

and with the help of these values we find

$$c' = c - a' + \frac{(a + a') \, (\varkappa' \omega' l - \varkappa \, \omega \, l')}{\varkappa' \omega' l + \varkappa \, \omega \, l'}.$$

Hence the electroscopic force of the circuit in the part P is expressed by the equation

$$u = \frac{(a + a')\,\varkappa'\,\omega' x}{\varkappa'\,\omega'\,l + \varkappa\,\omega\,l'} + c,$$

and that in the part P′ by the equation

$$u' = \frac{(a + a')\,\varkappa\,\omega\,x - \varkappa\,\omega\,l + \varkappa'\,\omega'\,l}{\varkappa'\,\omega'\,l + \varkappa\,\omega\,l'} - a + c.$$

If we substitute λ and λ' for $\dfrac{l}{\varkappa\,\omega}$ and $\dfrac{l'}{\varkappa'\,\omega'}$, the following more simple form may be given to these equations:—

$$\left.\begin{aligned} u &= \frac{a + a'}{\lambda + \lambda'} \cdot \frac{x}{\varkappa\,\omega} + c \\ u' &= \frac{a + a'}{\lambda + \lambda'}\left(\frac{x - l}{\varkappa'\,\omega'} + \frac{l}{\varkappa\,\omega}\right) - a' + c \end{aligned}\right\} \text{(L)}.$$

From the form of these equations it will be immediately perceived, that when the conductibility, or the magnitude of the section, is the same in both parts, the expressions for u and u' undergo no other change than that the letter representing the conductibility or the section entirely disappears.

17. We will now proceed to the consideration of a galvanic circuit, composed of three distinct parts P, P′, and P″, which case comprises the hydro-circuit.

If we represent by u, u', u'' respectively the electroscopic forces of the parts P, P′, and P″, then, according to § 15, the case there mentioned being here thrice repeated, we have, in accordance with the equation (c) there found, with respect to the part P,

$$u = f x + c,$$

with respect to the part P′,

$$u' = f' x + c',$$

and with respect to the part P″,

$$u'' = f'' x + c'',$$

where f, f', f'', c, c', c'' may represent any constant magnitudes remaining to be determined from the nature of the problem, and each equation has only so long any meaning as the abscissæ refer to that part to which the equations appertain. If we suppose the origin of the abscissæ at that extremity of the part P, which is connected with the part P″, and choose the direction of the abscissæ so that they lead from the part P to that of P′, and from thence into P″; if we further respectively

designate by l, l', and l'' the lengths of the parts P, P′, P″; and lastly, let u''_2 and u_1 represent the values of u'' and u at the place of contact where $x = o$, and u_2 and u'_1 the values of u and u' at the place of contact where $x = l$, and u'_2 and u''_1 the values of u' and u'' at the place of contact where $x = l + l'$, then we obtain

$$\begin{array}{ll} u''_2 = f''(l + l' + l'') + c'' & u_1 = c \\ u_2 = f\,l + c & u'_1 = f'\,l + c' \\ u'_2 = f'(l + l') + c' & u''_1 = f''(l + l') + c''. \end{array}$$

If we call a the tension which occurs at the place of contact where $x = o$, a' that at the place of contact where $x = l$, and a'' that at the place of contact where $x = l + l'$, we obtain, if we pay due attention to the general rule stated in the preceding paragraph,

$$\begin{aligned} a &= f''(l + l' + l'') + c'' - c \\ a' &= f\,l - f'\,l + c - c' \\ a'' &= f'(l + l') - f''(l + l') + c' - c'', \end{aligned}$$

and hence

$$a + a' + a'' = f\,l + f'\,l' + f''\,l''.$$

But from the considerations developed in § 13, when $\varkappa$ and ω represent the power of conduction and the section for the part P, $\varkappa'$ and ω' the same for the part P′, and $\varkappa''$ and ω'' for the part P″, at the individual places of contact, the following conditional equations are obtained:

$$\varkappa\,\omega\left(\frac{d\,u}{d\,x}\right) = \varkappa'\,\omega'\left(\frac{d\,u'}{d\,x}\right) = \varkappa''\,\omega''\left(\frac{d\,u''}{d\,x}\right),$$

where $\left(\frac{d\,u}{d\,x}\right)$, $\left(\frac{d\,u'}{d\,x}\right)$, $\left(\frac{d\,u''}{d\,x}\right)$ represent the particular values of $\frac{d\,u}{d\,x}$, $\frac{d\,u'}{d\,x}$, $\frac{d\,u''}{d\,x}$, belonging to the places of contact. From the equations stated at the commencement of the present paragraph for the determination of the electroscopic force in the single parts of the circuit, we obtain for every admissible value of x,

$$\frac{d\,u}{d\,x} = f, \quad \frac{d\,u'}{d\,x} = f', \quad \frac{d\,u''}{d\,x} = f'',$$

by which the preceding conditional equations are converted into

$$\varkappa\,\omega f = \varkappa'\,\omega' f' = \varkappa''\,\omega'' f''.$$

From these, and the equation between f, f', and f'' above de-

duced from the tensions, we now find, when λ, λ', λ'' are respectively substituted for $\frac{l}{\varkappa\,\omega}$, $\frac{l'}{\varkappa'\,\omega'}$, $\frac{l''}{\varkappa''\,\omega''}$,

$$f = \frac{a + a' + a''}{\lambda + \lambda' + \lambda''} \cdot \frac{1}{\varkappa\,\omega},$$

$$f' = \frac{a + a' + a''}{\lambda + \lambda' + \lambda''} \cdot \frac{1}{\varkappa'\omega'},$$

$$f'' = \frac{a + a' + a''}{\lambda + \lambda' + \lambda''} \cdot \frac{1}{\varkappa''\,\omega''},$$

and by the aid of these values we find further,

$$c' = \frac{a + a' + a''}{\lambda + \lambda' + \lambda''}\left(\frac{l}{\varkappa\,\omega} - \frac{l}{\varkappa'\omega'}\right) - a' + c,$$

$$c'' = \frac{a + a' + a''}{\lambda + \lambda' + \lambda''} \cdot \left(\frac{l}{\varkappa'\,\omega'} - \frac{l + l'}{\varkappa''\,\omega''} + \frac{l}{\varkappa\,\omega}\right) - (a' + a'') + c.$$

By substituting these values, we obtain for the determination of the electroscopic force of the circuit in the parts P, P′, P″ respectively, the following equations:

$$\left.\begin{aligned}
u &= \frac{a + a' + a''}{\lambda + \lambda' + \lambda''} \cdot \frac{x}{\varkappa\,\omega} + c \\
u' &= \frac{a + a' + a''}{\lambda + \lambda' + \lambda''} \cdot \left(\frac{x - l}{\varkappa'\,\omega'} + \frac{l}{\varkappa\,\omega}\right) - a' + c \\
u'' &= \frac{a + a' + a''}{\lambda + \lambda' + \lambda''} \cdot \left(\frac{x - (l + l')}{\varkappa''\,\omega''} + \frac{l'}{\varkappa'\,\omega'} + \frac{l}{\varkappa\,\omega}\right) - (a' + a'') + c
\end{aligned}\right\} (\mathrm{L}').$$

and it is easy to see, that these equations, with the omission of the letter $\varkappa$ or ω (both where they are explicit, as well as in the expressions for λ, λ', λ''), are the true ones for the case $\varkappa = \varkappa'$, or $\omega = \omega' = \omega''$.

18. These few cases suffice to demonstrate the law of progression of the formulæ ascertained for the electroscopic force, and to comprise them all in a single general expression. To do this with the requisite brevity, for the sake of a more easy and general survey, we will call the quotients, formed by dividing the length of any homogeneous part of the circuit by its power of conduction and its section, *the reduced length* of this part; and when the entire circuit comes under consideration, or a portion of it, composed of several homogeneous parts, we understand by its reduced length the sum of the reduced lengths of all its parts. Having premised this, all the previously found expressions for the electroscopic force, which are given by the equa-

tions (L) and (L′), may be comprised in the following general statement, which is true when the circuit consists of any number of parts whatever.

The electroscopic force of any place of a galvanic circuit, composed of any number of parts, is found by dividing the sum of all its tensions by its reduced length, multiplying this quotient by the reduced length of the part of the circuit comprised by the abscissa, and subtracting from this product the sum of all the tensions abruptly passed over by the abscissa; lastly, by varying the value thus obtained by a constant magnitude to be determined elsewhere.

If, therefore, we designate by A the sum of all the tensions of the circuit, by L its entire reduced length, by y the reduced length of the part which the abscissa passes through, and by O the sum of all the tensions to the points to which the abscissa corresponds, lastly, by u, the electroscopic force of any place in any part of the circuit, then

$$u = \frac{A}{L} y - O + c,$$

where c represents a constant, but yet undetermined magnitude.

Thus transformed, this exceedingly simple expression for the electroscopic force of any circuit will allow us hereafter to combine generality with conciseness, for which purpose we will, moreover, indicate by y the *reduced abscissa*. This form of the equation has besides the peculiar advantage that, without anything further, it is even applicable when in any part of the circuit the tensions and conductibilities constantly vary; for in this case we should merely have to take, instead of the sums, the corresponding integrals, and to define their limits according as the nature of the expression required. Since O does not change its value within the entire extent of the same homogeneous part of the circuit, and y constantly varies to the same amount on like portions of this extent, the following properties, already demonstrated less generally with respect to the simple circuit, evidently apply to every galvanic circuit, and in these is expressed the main character of galvanic circuits:—

a. The electric force of each homogeneous portion of the circuit varies throughout its entire length constantly, and on like extents always to the same amount; but where it ceases and another commences, it suddenly

changes to the extent of the entire tension situated at that place.

b. If any single place of the circuit is induced by any circumstance whatsoever to change its electric condition, all the other places of the circuit change theirs at the same time, and the same amount.

The constant c is in the rule determined by ascertaining the electroscopic force at any place of the circuit. If, for instance, we designate by u' the electroscopic force at a place of the circuit, the reduced abscissa of which is y', then, in accordance with the general equation above stated,

$$u' = \frac{A}{L} y' - O' + c,$$

where O' represents the sum of the tensions abruptly passed over by the abscissa y'. If we now subtract this equation, valid for a certain place of the circuit, from the previous one belonging in the same manner to all places, we obtain

$$u - u' = \frac{A}{L} (y - y') - (O - O'),$$

in which nothing more remains to be determined.

If the circuit, during its production, is exposed to no external deduction or adduction, the constant c must be sought for in the circumstance that the sum of all the electricity in the circuit must be zero. This determination is founded on the fundamental position, that, from a previously indifferent state, both electricities constantly originate at the same time and in like quantity. To illustrate, by an example, the mode in which the constant c is found in such a case, we will again consider the case treated of in § 16. In the portion P of that circuit, u is generally $= \frac{A}{L} y + c$, where $y = \frac{x}{\varkappa \omega}$, and in the portion P′ we have constantly $u = \frac{A}{L} y - a' + c$, where $y = \frac{x - l}{\varkappa' \omega'} + \lambda$. Since now the magnitude of the element, in the portion P, is $\omega\, d x$ or $\varkappa\, \omega^2\, d y$, but in the portion P′ is $\omega'\, d x$ or $\varkappa'\, \omega'^2\, d y$, we obtain for the quantity of electricity contained in an element of the first portion

$$\varkappa\, \omega^3\, d y \left(\frac{A}{L} y + c\right),$$

and for the quantity contained in an element of the second portion

$$\varkappa' \omega'^2 \, d y \left(\frac{A}{L} y - a' + c\right).$$

If we now integrate the first of the two preceding expressions from $y = O$ to $y = \lambda$, we then obtain for the whole quantity of electricity contained in the part P,

$$\varkappa \, \omega^2 \left[\frac{A}{2 L} \lambda^2 + c \lambda\right];$$

in the same manner we obtain, by integrating the second expression from $y = \lambda$ to $y = \lambda + \lambda'$, for the entire quantity of electricity contained in the portion P'

$$\varkappa' \omega'^2 \left[\frac{A}{2 L} (\lambda'^2 + 2 \lambda \lambda') - a' \lambda' + c \lambda'\right].$$

But the sum of the two last found quantities must, in accordance with the above-advanced fundamental position, be zero. We thus obtain the equation required for the determination of the constant c, and it only remains to be observed that λ and λ' are the reduced lengths corresponding to the portions P and P'.

We have hitherto always tacitly supposed only positive abscissæ. But it is easy to be convinced that negative abscissæ may be introduced quite as well. For let $-y$ represent such a negative reduced abscissa for any place of the circuit, then $L - y$ is the positive reduced abscissa pertaining to the same place, for which the general equation found is valid; we accordingly obtain

$$u = \frac{A}{L} (L - y) - O + c$$

or

$$u = -\frac{A}{L} y - (O - A) + c.$$

But $O - A$ evidently expresses, if regard be had to the general rule expressed in § 16, the sum of all the tensions abruptly passed over by the negative abscissa, whence it is evident that the equation still retains entire its former signification for negative abscissæ.

19. If we imagine one of the parts of which the galvanic circuit is composed to be a non-conductor of electricity, *i. e.* a body whose capacity of conduction is zero, the reduced length of the entire circuit acquires an indefinitely great value. If we now make it a rule never to let the abscissæ enter into the non-conducting part, in order that the reduced abscissa y may constantly retain a finite value, the general equation changes into the following:

$$u = -\dot{\mathrm{O}} + c,$$

which indicates that the electroscopic force in the whole extent of each other homogeneous portion of the circuit is everywhere the same, and merely changes suddenly from one part to the other to the amount of the entire tension prevailing at its place of contact.

To determine the constant c in this equation, we will suppose the electroscopic force, at any one place of the circuit, to be given. If we call this u', and the sum of the tensions there abruptly passed over by the abscissa O′, we have

$$u - u' = -(\mathrm{O} - \mathrm{O}').$$

The difference of the electroscopic forces of any two places of an open circuit, *i. e.* a galvanic circuit interrupted by a non-conductor, is consequently equal to the sum of all the tensions situated between the two places, and the sign which has to be placed before this sum is always easily to be determined from mere inspection.

20. We will now notice another peculiarity of the galvanic circuit, which merits especial attention. To this end let us keep in view exclusively one of the homogeneous parts of the circuit, and imagine, for the sake of simplicity, the origin of the abscissæ placed in one end of it, and the abscissæ directed towards the other end. If we designate its reduced length by λ, and the reduced length of the other portion of the circuit by Λ, then

$$u = \frac{\mathrm{A}}{\Lambda + \lambda} \cdot y + c$$

within the length λ; the following form may also be given to this equation:

$$u = \frac{\frac{\mathrm{A}\,\lambda}{\Lambda + \lambda}}{\lambda} \cdot y + c;$$

circuit
the ~~extent~~ is consequently similarly circumstanced to a simple homogeneous circuit, at whose ends the tension $\frac{\mathrm{A}\,\lambda}{\Lambda + \lambda}$ occurs. If, accordingly, A has a very sensible value, such as it can acquire in the voltaic pile, and if the ratio $\frac{\lambda}{\Lambda + \lambda}$ approaches to unity, then the tension $\frac{\mathrm{A}\,\lambda}{\Lambda + \lambda}$ will likewise be still very per-

ceptible; consequently its various gradations in the extent of the portion λ are very easily perceptible. This conclusion is of importance, because it affords the means of presenting to the senses the law of electric distribution even on compound circuits, when it is no longer possible on the simple circuit, on account of its extremely feeble force. It is, moreover, immediately evident, that, with equal tensions, this phænomenon will be indicated with greater intensity, the greater λ is in comparison with Λ.

21. A phænomenon common to all galvanic circuits is the sudden change to which its electroscopic force may incessantly, and arbitrarily, be subjected. This phænomenon has its source in the previously developed properties of such circuits. Since, as we have found, each place of a galvanic circuit undergoes the same alterations to which a single one is exposed, we have it in our power to give sometimes one, sometimes another value to the electroscopic force at any certain place. Among these changes those are the most remarkable which we are able to produce by deductive contact, *i. e.* by destroying the electroscopic force sometimes at one, and sometimes at another place of the circuit; its magnitude, however, has its natural limits in the magnitude of the tensions.

There is another class of phænomena which is immediately connected with these. If, for instance, we call r the space over which the electroscopic force is diffused in a given galvanic circuit, u the electroscopic force of the circuit at one of its points, which is immediately connected with an external body M, and u' the electroscopic force of the same circuit at the same place as it was previous to contact with the body M, $u' - u$ is evidently the alteration in the electroscopic force produced at this place; consequently, since this change likewise occurs uniformly at all the other places of the circuit, $r(u' - u)$ is the quantity of electricity which the change produced over the entire circuit comprises, and accordingly that which has passed over into the body M. If now we suppose that in the state of equilibrium the electroscopic force is everywhere of equal intensity at all places of the body M in which it occurs, and represent by R the space over which it is diffused in the body M, then its electroscopic force is evidently $\frac{r(u' - u)}{R}$. But this force is in the state of equilibrium equal to the u', which the

place of the circuit, brought into contact with M, has assumed when no new tension originates at this place of contact; under this supposition therefore

$$u = \frac{r\,(u' - u)}{\mathrm{R}},$$

whence we find

$$u = \frac{r\,u'}{r + \mathrm{R}}.$$

From this equation it results that the electroscopic force in the body M will constantly be smaller than it was at the touched place before contact; and also that both will approximate the more to each other, the greater r is in comparison to R. If we regard R as a constant magnitude, the relation of the electroscopic forces u and u' to each other depends solely upon the magnitude of the space which the electricity occupies in the circuit; we can therefore bring the electroscopic force of the body M nearer to its greatest value solely by increasing the capacity of the circuit, either by a general increase of its dimensions, or by attaching anywhere to it foreign masses. Upon the nature of these masses, when they are merely conductors of electricity, and do not give rise to new tensions, none of this effect, in my opinion, depends, but solely upon their magnitudes. If the attached masses occupy an infinitely great space, which case occurs when the circuit has anywhere a complete deduction, then the electroscopic force in the body M will constantly be equal to that which the place of the circuit touched by it possesses.

To connect these effects with the action of the condenser, we have merely to bear in mind, that a condenser, whose magnitude is R, and whose number of charges is m, must be considered equal to a common conductor of the magnitude m R, yet with the difference that its electroscopic force is m times that of the common conductor. If, therefore, we designate by u the electroscopic force of the condenser, which is brought into connexion with a place of the circuit whose force is u', we obtain

$$u = \frac{m\,r\,u'}{r + m\,\mathrm{R}},$$

whence it follows that the condenser will indicate m times the force of the touched place when r is very great in comparison with m R; but that it will have a weakening action so soon as r is

equal to, or smaller than R. Masses attached anywhere to the circuit will accordingly make the indications of the condenser approximate to its maximum in proportion as they are greater, and a circuit touched at any place will constantly produce in the condenser the maximum of increase.

The preceding determinations suppose that one plate of the condenser remains constantly touched deductively. We will now take into consideration the case where the two plates of an insulated condenser are connected with various points of a galvanic circuit. In the first place, it is evident that the two plates of the condenser will assume the same difference of free electricity which the various places of the circuit with which they are in contact require unconditionally, from the peculiar nature of galvanic actions. Consequently, if d represents the difference of the electroscopic force at the two places of the circuit, and u the free electricity of one plate of the condenser, then $u + d$ is the free electricity of the other plate, and everything will depend on finding, from the known free electricities existing in the plates of the condenser, those actually present in them. If, for this purpose, we call A the actual intensity of electricity in the plate, whose free electricity is $u + d$, then $A - u - d$ represents the portion retained in the same plate; in the same manner $B - u$ designates the portion of electricity retained in the plate, whose free electricity is u, when B represents the actual intensity of the electricity in this plate. If now we represent by n the relation between the electricity retained by one plate, and the actual electricity of the other plate, the following two equations arise:

$$A - u - d + n\,B = 0,$$
$$B - u + n\,A = 0,$$

from which the values A′ and B result, as follows:

$$A = \frac{d + u\,(1 - n)}{1 - n^2},$$

$$B = \frac{u\,(1 - n) - n\,d}{1 - n^2}.$$

But from the theory of the condenser, it is well known that $1 - n^2 = \frac{1}{m}$, if m is the number of charges of the condenser; if, therefore, we substitute $\frac{1}{m}$ for $1 - n^2$ in the expressions for

A and B, and at the same time $1 - \frac{1}{2m}$ for n, which is permitted when m, as is usually the case, denotes a very large number, we obtain

$$A = m\,d + \tfrac{1}{2}\,u,$$
$$B = -m\,d + \tfrac{1}{2}\,u + \tfrac{1}{2}\,d.$$

Or when m is a very large number, and n not much greater than d, we may, without committing any perceptible error, place

$$A = m\,d,$$
$$B = -m\,d,$$

in which is expressed the known law, that when two different places of a voltaic pile are brought into connexion with the two plates of an insulated condenser, each plate takes the same charge as if the other plate, and the corresponding place of the pile, had been touched deductively. At the same time our considerations show that this law ceases to be true when u can no longer be regarded as evanescent towards $m\,d$. This case would occur if, for instance, two places, near the insulated upper pole of a voltaic pile, constructed of a great number of elements, came in contact with the plates of the condenser, while the inferior pole of this pile remained in deductive connexion with the earth.

The determinations hitherto given respecting the mode in which the galvanic circuit imparts its electricity to foreign bodies, and which appear to me to leave nothing more to be wished for in the explanation of this subject, might, however, give rise to researches of a very different kind, and of no slight interest. For it is placed beyond all doubt, both from theoretical considerations, as well as from experiments, that electricity in motion penetrates into the interior of bodies, and its quantity accordingly depends on the space occupied by the bodies; while, on the other hand, it is no less ascertained that static electricity accumulates at the surface of bodies, and its quantity therefore is dependent on the extent of surface. But it would hence result, that in the closed galvanic circuit, r in the above formulæ would express the volume of the circuit; in the open circuit, on the contrary, the magnitude of its surface, on which point, in my opinion, experiments might decide without great difficulty.

22. We have hitherto kept in view a circuit on which the surrounding atmosphere exercised no influence, and which has

already arrived at its permanent state, and we have treated it at a length which it merits from the abundance and importance of the phænomena connected with it. However, not to let even here the other circuits pass entirely unnoticed, we will briefly indicate the method to be pursued for the most simple case, and thus point out the path to be followed, although only at a distance.

If it is intended to take into consideration the influence of the atmosphere on the galvanic circuit, the member $\frac{b\,c}{\omega}\,u$ must be added to the member $\varkappa\,\frac{d^2\,u}{d\,x^2}$ of the equation (a) in § 11, we then obtain for the circuit which has acquired a permanent state, for which $\frac{d\,u}{d\,t} = 0$, the equation

$$0 = \varkappa\,\frac{d^2\,u}{d\,x^2} - \frac{b\,c}{\omega}\,u;$$

or if we put $\frac{b\,c}{\varkappa\,\omega} = \beta^2$,

$$0 = \frac{d^2\,u}{d\,x^2} - \beta^2\,u.$$

The integral of this equation is

$$u = c\,.\,e^{\beta\,x} + d\,.\,e^{-\beta\,x},$$

where e represents the base of the natural logarithms, and c, d any constants to be determined from the other circumstances of the problem.

If we now call $2\,l$ the length of the entire circuit, and fix the origin of the abscissæ in that place of the circuit which is equidistant from the point of excitation; if, further, we designate by a the tension existing at the point of excitation, we obtain

$$a = (c - d)\,(e^{\beta\,l} - e^{-\beta\,l}).$$

If we write the previously found equation thus,

$$u = (c - d)\,e^{\beta\,x} + d\,(e^{\beta\,x} + e^{-\beta\,x}),$$

and substitute for $c - d$, the value just ascertained, we have

$$u = \frac{a\,.\,e^{\beta\,x}}{e^{\beta\,l} - e^{-\beta\,l}} + d\,(e^{\beta\,x} + e^{-\beta\,x}).$$

If we now suppose for the determination of the other constant, that the sum of the two electroscopic forces, situated at the

point of excitation, is known, and is equal to b, which case always occurs when the electroscopic force of the circuit is given at any one of its places, we obtain

$$b = \frac{a\,(e^{\beta l} + e^{-\beta l})}{e^{\beta l} - e^{-\beta l}} + 2\,d\,(e^{\beta l} + e^{-\beta l});$$

and after substitution and proper reduction,

$$u = \frac{\frac{1}{2}\,a\,(e^{\beta x} - e^{-\beta x})}{e^{\beta l} - e^{-\beta l}} + \frac{\frac{1}{2}\,b\,(e^{\beta x} + e^{-\beta x})}{e^{\beta l} + e^{-\beta l}},$$

which for $b = 0$, *i. e.* for a circuit left entirely to itself, changes into

$$u = \frac{\frac{1}{2}\,a\,(e^{\beta x} - e^{-\beta x})}{e^{\beta l} - e^{-\beta l}}.$$

These equations, which hold for a circuit homogeneous and prismatic in its whole extent, change when $\beta = 0$ again into the above, where the influence of the atmosphere on the circuit was, under the circumstances given above, left out of consideration. Since $\beta^2 = \frac{b}{\varkappa} \cdot \frac{c}{\omega}$, it follows that the influence of the atmosphere on the galvanic circuit must be less, the smaller the conducting power of the atmosphere is in comparison to that of the circuit, and the smaller the quotient $\frac{c}{\omega}$ is. But the quotient $\frac{c}{\omega}$ expresses the relation of the surface of a disc of the conductor surrounded by the atmosphere to the volume of the same disc, and it might therefore appear that $\frac{c}{\omega}$ must constantly be infinitely small. However, it must not be forgotten that we have not here to deal with mathematical, but with physical determinations; for, strictly taken, c does not represent a surface, but that portion of a disc of the circuit on which the atmosphere has direct influence, and ω in fact signifies nothing more than that part of a disc of the circuit which is traversed by the electricity continually passing through the circuit. In general, therefore, c is indeed incomparably smaller than ω; but where the electric current can only move forwards with the greatest difficulty, and on that account but very slowly, as is more or less the case in dry piles, the magnitude c may, in accordance with what was stated in the preceding paragraph, become very nearly equal to ω; for undoubtedly a gradual transition,

modified by the cotemporaneous circumstances, must occur from that which is peculiar to the rapid current to that belonging to the state of perfect equilibrium. Here, then, is a wide field open for future researches.

23. In cases where the permanent state is not instantaneously assumed, as it usually is in dry piles, we should, in order to become acquainted with the changes of the circuit up to that period, proceed from the complete equation

$$\gamma \frac{d u}{d t} = \varkappa \frac{d^2 u}{d x^2} - \frac{b c}{\omega} u, \qquad (*)$$

because in this case we cannot consider $\frac{d u}{d t} = 0$, and the member $\frac{b c}{\omega} u$ must either remain in it, or be removed from it, according to whether it is considered worth while to take the influence of the atmosphere on the circuit into consideration or not. If we again place, as in the previous paragraph, $\beta^2 = \frac{b c}{\varkappa \omega}$, and, also $\frac{\varkappa}{\gamma} = \varkappa'$, the preceding equation changes into the following:

$$\frac{d u}{d t} = \varkappa' \left(\frac{d^2 u}{d x^2} - \beta^2 u\right),$$

and we immediately perceive, that on admitting $\beta = 0$, the action of the atmosphere is left out of the question.

In the present case u represents a function of x and t, which, however, in proportion as the time t increases, becomes gradually less dependent on t, and at last passes over into a mere function of x, which expresses the permanent state of the circuit, with the nature of which we have already become acquainted. If we designate this latter function by u', and place $u = u' + v$, then v is evidently a function of x and t, which indicates every deviation of the circuit from its permanent state, and consequently after the lapse of a certain time entirely disappears. If we now substitute $u' + v$ for u in the equation $(*)$, and bear in mind that u' is independent of t, and of such nature that

$$0 = \frac{d^2 u'}{d x^2} - \beta^2 u',$$

the equation

$$\frac{d v}{d t} = \varkappa' \left(\frac{d^2 v}{d x^2} - \beta^2 v\right) \qquad (\mathfrak{D})$$

then remains for the determination of the function v, which still possesses the same form as the equation ($*$), but differs from it in this respect, that v is a function of x, and t of a different nature from u, by which its final determination is much facilitated.

The integral of the equation (☽), in the form in which it was first obtained by Laplace, is

$$v = \frac{e^{-\varkappa' \beta^2 t}}{\sqrt{\pi}} \int e^{-y^2} f(x + 2y \sqrt{\varkappa' t})\, dy, \qquad (☿)$$

where e represents the base of the natural logarithms, π the ratio of the circumference of a circle to its diameter, and f an arbitrary function to be determined from the peculiar nature of each problem, while the limits of the integration must be taken from $y = -\infty$ to $y = +\infty$. For $t = 0$ we have $v = f x$, because between the indicated limits $\int e^{-y^2} dy = \sqrt{\pi}$, whence it results that if we know how to find the function v in the case where $f = 0$, we should thereby likewise discover $f x$, consequently the arbitrary function f. Now in general $v = u - u'$; but if we reckon the time t from the moment when, by the contact at the two extremities of the circuit, the tension originates, then u, when $t = 0$ has evidently fixed values only at these extremities, at all other places of the circuit u is $= 0$; accordingly, in the whole extent of the circuit $v = -u'$ in general when $t = 0$; only at the extremities of the circuit at the same time $v = u - u'$. If, therefore, we imagine a circuit left from the first moment of contact entirely to itself, then v constantly $= 0$ at its extremities, so that therefore in the interior of the circuit $v = -u'$, when $t = 0$, and at its extremities $v = 0$. Since, in accordance with our previous inquiries, u' may be regarded as known for each place of the circuit, this likewise applies to v when $t = 0$; we know then the form of the arbitrary function $f x$, so long as x belongs to a point in the circuit.

However, the integral given for the determination of v requires the knowledge of the function $f x$ for all positive and negative values of x; we are thus compelled to give, by transformation, such as the researches respecting the diffusion of heat have made us acquainted with, such a form to the above equation that only pre-supposes the knowledge of the function $f x$ for the extent of the circuit. The transformation applicable to

the present case gives, when $2\,l$ designates the length of the circuit, and the origin of the abscissæ is placed in its centre*,

$$v = \frac{e^{-\varkappa'\beta^2 t}}{l}\left[\Sigma\left(e^{\frac{-\varkappa' i^2\pi^2 t}{l^2}} . \sin\frac{i\pi x}{l}\int \sin\frac{i\pi y}{l} f y\, d y\right)\right.$$
$$\left. + \Sigma\left(e^{\frac{-(2i-1)^2\pi^2 t}{4l^2}}\cos\frac{(2i-1)\pi x}{2l}\int\cos\frac{(2i-1)\pi y}{2l} f y\, d y\right)\right]$$

where the sums must be taken from $i = 1$ to $i = \infty$, and the integrals from $y = -l$ to $y = +l$. If we now substitute in this equation for $f x$ its value $-u'$, whereby according to our supposition in the preceding paragraph, if a represents the tension at the place of contact,

$$u' = \frac{\frac{1}{2}a\,(e^{\beta x} - e^{-\beta x})}{e^{\beta l} - e^{-\beta l}},$$

and then integrate, we obtain, since between the indicated limits

$$\tfrac{1}{2}a\int \sin\frac{i\pi y}{l} . \frac{e^{\beta y} - e^{-\beta y}}{e^{\beta l} - e^{-\beta l}} . d y = -\frac{a\, i\pi l \cos i\pi}{i^2\pi^2 + \beta^2 l^2},$$

and

$$\tfrac{1}{2}a\int \frac{e^{\beta y} - e^{-\beta y}}{e^{\beta l} - e^{-\beta l}} . \cos\frac{(2i-1)\pi y}{2l} . d y = 0,$$

for the determination of v the equation

$$v = a . e^{-\varkappa'\beta^2 t}\,\Sigma\left(\frac{i\pi \sin\frac{i\pi(l+x)}{l}}{i^2\pi^2 + \beta^2 l^2} . e^{\frac{-\varkappa'\pi^2 i^2 t}{l^2}}\right),$$

and, lastly, since $u = u' + v$

$$u = \frac{\frac{1}{2}a\,(e^{\beta x} - e^{-\beta x})}{e^{\beta l} - e^{-\beta l}} + a . e^{-\varkappa'\beta^2 t} \times \Sigma\left(\frac{i\pi \sin\frac{i\pi(l+x)}{l}}{i^2\pi^2 + \beta^2 l^2} . e^{\frac{-\varkappa'\pi^2 i^2 t}{l^2}}\right),$$

which equation, for $\beta = 0$, *i. e.* when it is not intended to take into consideration the influence of the atmosphere, passes into

$$u = \frac{a}{2l}x + a\,\Sigma\left(\frac{1}{i\pi}\sin\frac{i\pi(l+x)}{l} . e^{\cancel{\frac{-\varkappa'\pi^2 i^2 t}{l^2}}}\right). \quad e^{\frac{-\varkappa'\pi^2 t}{l^2}}$$

It is easily perceived that the value of the second member to the right in the equations which have been found for the determination of u, becomes smaller and smaller as the time increases,

* See *Journal de l'Ecole Polytechnique*, cap. xix. p. 53.

and that it at last entirely vanishes; the permanent state of the circuit has then occurred. This moment can, as is evident from the form of the expression, be retarded by a diminished power of conduction, and in a far greater degree by an increased length of the circuit.

This expression found for u, however, holds perfectly only so long as the circuit, which we have supposed, is not induced by any external disturbance to change its natural state. If the circuit is at any time compelled by any external cause, for instance, by deductive contact at any place, to approximate to an altered permanent state, the above method has to undergo some changes, which I intend to develope on another occasion. I will, moreover, observe, that it is in this last class of galvanic circuits, in which the peculiar phænomena of dry piles, and, in general, of circuits of unusually great length, have to be sought for; to which class likewise belong the circuits of very great length employed in the experiments of Basse, Erman, and Aldini, if the influence of their great length be not annulled by an increased goodness of conduction, or by an increased section.

C. *Phænomena of the Electric Current.*

24. According to what was advanced in paragraph 12, the magnitude of the electric current, in a prismatic body, will in general be expressed for each of its places by the equation

$$S = \omega \varkappa \frac{d u}{d x},$$

where S denotes the magnitude of the current, and u the electroscopic force at that place of the circuit whose abscissa is x, while ω represents the section of the prismatic body, and $\varkappa$ its power of conduction at the same place. To connect this equation with the general equation found in § 18 for any circuit, composed of any number of parts, we write it thus:

$$S = \varkappa \omega \frac{d u}{d y} \cdot \frac{d y}{d x},$$

and substitute for $\frac{d u}{d y}$ the value $\frac{A}{L}$ resulting from that general equation, and for $\frac{d y}{d x}$ the value $\frac{1}{\varkappa \omega}$ easily deducible from the same paragraph, both which values are valid for each place,

situated between two points of excitation, we then very simply obtain

$$S = \frac{A}{L},$$

where L denotes the entire reduced length of the circuit, and A the sum of all its tensions. By means of this equation we obtain the magnitude of the electric current of a galvanic circuit, composed of any number of prismatic parts, which has acquired its permanent state, which is not affected by the surrounding atmosphere, and the single sections of which possess in all their points one and the same electroscopic force; in this category are comprised the most frequently occurring cases, on which account we shall dissect this result in the most careful manner.

Since A represents the sum of all the tensions in the circuit, and L the sum of the reduced lengths of all the individual parts, there results, in the first place, from the equation found, the following general properties relative to the electric current of the galvanic circuit.

I. The electric current is decidedly of equal magnitude at all places of a galvanic circuit, and is independent of the value of the constant c, which, as we have seen, fixes the intensity of the electroscopic force at a determined place. In the open circuit the current ceases entirely, for in this case the reduced length L acquires an infinitely great value.

II. The magnitude of the current, in a galvanic circuit, remains unchanged when the sum of all its tensions and its entire reduced length are varied, either not at all, or in the same proportion; but it increases, the reduced length remaining the same, in proportion as the sum of the tensions increases, and the sum of the tensions remaining the same, in proportion as the reduced length of the circuit diminishes. From this general law we will, moreover, particularly deduce the following.

1. A difference in the arrangement and distribution of the individual points of excitation, by a transposition of the parts of which the circuit consists, has no influence on the magnitude of the current when the sum of all the tensions remains the same. Thus, for instance, the current would remain unaltered in a circuit formed in the order copper, silver, lead, zinc, and a fluid, even when the silver and lead

change places with each other; because, according to the laws of tension observed with respect to metals, this transposition would, it is true, alter the individual tensions, but not their sum.

2. The intensity of a galvanic current continues the same, although a part of the circuit be removed, and another prismatic conductor be substituted in its place, only both must have the same reduced length, and the sum of the tensions in both cases remain the same; and *vice versâ*, when the current of a circuit is not altered by the substitution of one of its parts for a foreign prismatic conductor, and we can be convinced that the sum of the tensions has remained the same, then the reduced lengths of the two exchanged conductors are equal.

3. If we imagine a galvanic circuit always constructed of a like number of parts, of the same substance, and arranged in the same order, in order that the individual tensions may be regarded as unchangeable, the current of this circuit increases, the length of its parts remaining unaltered, in the same proportion in which the sections of all its parts increase in a similar manner, and the sections remaining unaltered, in the same proportion in which the length of all its parts uniformly decrease. When the reduced length of a part of the circuit far exceeds that of the other parts, the magnitude of the current will principally depend on the dimensions of this part; and the law here enounced will assume a much more simple form, if, in the comparison, attention be solely directed to this one part.

The conclusion arrived at in II. 2. presents a convenient means for the determination of the conductibility of various bodies. If, for instance, we imagine two prismatic bodies, whose lengths are l and l', their sections respectively ω and ω', and whose powers of conduction are $\varkappa$ and $\varkappa'$, and both bodies possess the property of not altering the current of a galvanic circuit when they alternatively form a portion of it, and both leave the individual tensions of the circuit unchanged, then

$$\frac{l}{\varkappa\,\omega} = \frac{l'}{\varkappa'\,\omega'},$$

consequently

$$\varkappa : \varkappa' = \frac{l}{\omega} : \frac{l'}{\omega'};$$

the powers of conduction, therefore, of both bodies are directly proportionate to their lengths, and inversely proportionate to their sections. If it is intended to employ this relation in the determination of the powers of conduction of various bodies, and we choose for the experiments prismatic bodies of the same section, which indeed is requisite for the sake of great accuracy, their lengths will enable us to determine accurately their conductibilities.

25. In the preceding paragraph we have deduced the magnitude of the current from the general equation given in § 18,

$$u = \frac{A}{L} y - O + c,$$

and have found that it is expressed by $\frac{A}{L}$, the coefficient of y.

To ascertain the value $\frac{A}{L}$ it is in general requisite to possess an accurate knowledge of all the single parts of the circuit, and their reciprocal tensions; but our general equation indicates a means of deducing this value likewise from the nature of any single part of the circuit in the state of action, which we will not disregard, as it will be of great service to us hereafter. If, namely, we conceive in the above equation y to be increased by any magnitude Δy, and designate by ΔO the corresponding change of O, and by Δu that of u, there results from that equation

$$\Delta u = \frac{A}{L} \Delta y - \Delta O,$$

and we thence find

$$\frac{A}{L} = \frac{\Delta u + \Delta O}{\Delta y};$$

we find, therefore, the magnitude of the electric current by adding to the difference of the electroscopic forces at any two places of the circuit the sum of all the tensions situated between these two places, and dividing this sum by the reduced length of the part of the circuit which lies between these same places. If there should be no tension within this portion of the circuit, then $\Delta O = 0$, and we obtain

$$\frac{A}{L} = \frac{\Delta u}{\Delta y}.$$

26. The voltaic pile, which is a combination of several similar

simple circuits, merits peculiar attention in this place, from the numerous and varied experimental results obtained by its means.

If A represent the sum of the tensions of a closed galvanic circuit, and L its reduced length, the magnitude of its current is, as we have found,

$$\frac{A}{L}.$$

Now, if we imagine n such circuits perfectly similar to the former, but open, and constantly bring the end of each one in direct connexion with the commencement of the next following one, in such a manner that between each two circuits no new tension occurs, and all the previous tensions remain afterwards as before, then the magnitude of the current of this voltaic combination, closed in itself, is evidently

$$\frac{nA}{nL},$$

consequently equal to that in the simple circuit. This equality of the circuit, however, no longer exists when a new conductor, which we will call the interposed conductor, is inserted in both. If, namely, we designate the reduced length of this interposed conductor by Λ, then, when no new tension is produced by it, the magnitude of the current in the simple circuit will be

$$\frac{A}{L+\Lambda},$$

and in the voltaic combination, consisting of n, such elements

$$\frac{nA}{nL+\Lambda} \quad \text{or} \quad \frac{A}{L+\frac{\Lambda}{n}};$$

therefore in the latter circuit it is constantly greater than in the former, and, in fact, a gradual transition takes place from equality of action, which is evinced when Λ disappears, to where the voltaic combination exceeds n times the action of the simple circuit, which case occurs when Λ is incomparably greater than n L. If by Λ we represent the relative length of the body upon which the circuit is to act by the force of its current, then from the observations just brought forward it results that it is most advantageous to employ a powerful simple circuit when Λ is very small in comparison to L; and, on the contrary, the voltaic pile, when Λ is very great in comparison with L.

But how must, in each separate case, a given galvanic apparatus be arranged so as to produce the greatest effect? Let us suppose, in solving this problem, that we possess a certain magnitude of surface; for instance, of copper and zinc, from which we can form, according to pleasure, a single large pair of plates, or any number of smaller pairs, but in the same proportion, and, moreover, that the liquid between the two metals is constantly the same, and of the same length, which latter supposition means nothing more than that the two metals between which the liquid is confined retain, under all circumstances, the same distance from each other.

Let Λ be the reduced length of the body upon which the electric current is to act, L the reduced length of the apparatus when formed into a simple circuit, and A its tension; then, when it is altered into a voltaic combination of x elements, its present tension will be x A, and the reduced length of each of its present elements x L, accordingly the reduced length of all the x elements x^2 L, consequently the magnitude of the action of the voltaic combination of x elements is

$$\frac{x\,\mathrm{A}}{x^2\,\mathrm{L} + \Lambda}.$$

This expression acquires its greatest value $\frac{\mathrm{A}}{2\sqrt{\Lambda\,.\,\mathrm{L}}}$ when $x = \sqrt{\frac{\Lambda}{\mathrm{L}}}$. We hence see that the apparatus in form of a simple circuit is most advantageous, so long as Λ is not greater than L; on the contrary, the voltaic combination is most useful when Λ is greater than L, and indeed it is best constructed of two elements when Λ is four times greater than L, of three elements when Λ is nine times greater than L, and so forth.

27. The circumstance that the current always remains the same at all places, affords us the means of multiplying its external action, as in the case when the current influences the magnetic needle. We will, for perspicuity, suppose that, in order to test the action of the current on the magnetic needle, each time a part of the circuit be formed into a circle of a given radius, and so placed in the magnetic meridian that its centre coincides with the point of rotation of the needle. Several such distinct coils, formed of the circuit in exactly the same manner, will, taken singly, produce, on account of the equality of the current in each, equally powerful effects on the magnetic

needle; if we imagine them, therefore, so arranged near one another, that though they are separated by a non-conducting layer, they are yet situated so close together that the position of each one toward the magnetic needle may be regarded as the same, they would produce a greater effect on the magnetic needle in proportion as their number increased. Such an arrangement is termed a *multiplier*.

Now, let A be the sum of the tensions of any circuit, and L its reduced length; let also Λ be the reduced length of one of the interposed conductors formed into a multiplier of n convolutions; then, if we represent the reduced length of one such convolution by λ, $\Lambda = n\,\lambda$, the action of the multiplier on the magnet needle will be proportional to the value

$$\frac{n\,A}{L + n\,\lambda}.$$

But the action of a similar coil of the circuit, without the multiplier, is, according to the same standard,

$$\frac{A}{L},$$

and we will suppose the portion of the circuit, whence the coil is taken, to be of the same nature as in the multiplier; accordingly the difference between the former and the present effect is

$$\frac{n\,L - (L + n\,\lambda)}{L + n\,\lambda} \cdot \frac{A}{L},$$

which is positive or negative according as $n\,L$ is greater or less than $L + n\,\lambda$. Consequently the action on the magnetic needle will be augmented or diminished by the multiplier formed of n coils, according as the n times reduced length of the circuit, without interposed conductor, is greater or less than the entire reduced length of the circuit with the interposed conductor.

If $n\,\lambda$ is incomparably greater than L, the action of the multiplier on the needle will be

$$\frac{A}{\lambda}.$$

To this value, which indicates the extreme limit of the action by means of the multiplier, whether it be strengthening or weakening, belong several remarkable properties, which we will briefly notice. It is constantly supposed that the multiplier is formed of so many coils that the magnitude of its action may,

without committing any sensible error, be considered equal to the limit value.

Since the action of a coil of the circuit is $\frac{A}{L}$, while the action of the multiplier, in connexion with the same circuit, is $\frac{A}{\lambda}$, it is evident that the two actions are in the same ratio to each other as the reduced length λ and L; if, therefore, we are acquainted with the two actions, and with one of the two reduced lengths, the other may be found, and in the same manner one of the two actions may be deduced from the other, and the two reduced lengths.

Since the limit of the action of the multiplier is $\frac{A}{\lambda}$, it increases when λ is invariable in the same proportion as the sum of the tensions A in the circuit increases; we may, therefore, by comparing the extreme actions of the same multiplier in various circuits, arrive at the determination of their relative tensions. At the same time we perceive that the extreme action of the multiplier increases, when several simple circuits are formed into a voltaic combination, and, indeed, in direct proportion to the number of the elements. In this manner it is always in our power, in cases where the multiplier in connexion with the simple circuit produces a weakening effect, to cause it to indicate any increase of force whatever.

If we call the actual length of a coil of the multiplier l, its conductibility $\varkappa$, and its section ω, then $\lambda = \frac{l}{\varkappa\,\omega}$, and consequently the extreme action of the multiplier

$$\varkappa\,\omega \,.\, \frac{A}{l},$$

whence it results that in the same circuit the extreme actions of two multipliers of coils of equal diameter, are in the ratio to each other of the products of their conductibility and their section. These extreme actions are, therefore, in two multipliers, which differ only in being formed of two distinct metals, in proportion to the conductibility of these metals; and when the multipliers consist of similar convolutions, and of one metal, their extreme actions are proportional to their sections.

But all these determinations are based upon the supposition that the action of a portion of the circuit on the magnetic

needle, under otherwise similar circumstances, is proportional to the magnitude of the current. But long since direct experiments have established the correctness of this supposition.

28. We will now proceed to the consideration of a multiple conduction existing at the same time. If, for instance, we imagine an open circuit, whose separated extremities are connected by several conductors, arranged by the side of each other, it may be asked, according to what law is the current distributed in the adjacent conductors? In answering this question, we might proceed directly from the considerations contained in § 11 to 13; but we shall more simply attain the required object from the peculiarity of galvanic circuits ascertained in § 25, in which case we will, for the sake of simplicity, suppose that none of the former tensions is destroyed by the opening of the circuit, nor a new tension produced by the conductor which is introduced.

For if λ, λ', λ'', &c. represent the reduced lengths of the conductors brought into connexion with the extremities of the open circuit, and α the difference of the electroscopic forces at the extremities of the circuit, after the conductors have been introduced, the same difference will also occur at the ends of the single adjacent conductors, since, according to the supposition we have made, no new tension is introduced by the conductor. Since now, according to § 13, the magnitude of the current in the circuit must be equal to the sum of all the currents in the adjacent conductors, we may imagine the circuit to be divided into as many parts as there are adjacent conductors; then, according to § 25, the magnitude of the current in each adjacent conductor, and in the corresponding part of the circuit, will respectively be

$$\frac{\alpha}{\lambda}, \quad \frac{\alpha}{\lambda'}, \quad \frac{\alpha}{\lambda''}, \text{ \&c.,}$$

whence, in the first place, it results that the magnitude of the current in each adjacent conductor is in inverse ratio to its reduced length. If we now imagine a single conductor of such nature, that, being substituted for all the adjacent conductors in the circuit, it does not at all alter its current; then, in the first place, α, according to § 25, must retain the same value, and, if we designate by Λ the reduced length of this conductor, must moreover be

$$\frac{1}{\Lambda} = \frac{1}{\lambda} + \frac{1}{\lambda'} + \frac{1}{\lambda''} + \&c.$$

From the preceding explanations we may conclude, that when A denotes the sum of all the tensions, and L the entire reduced length of the circuit without adjacent conductors, the magnitude of the current, while the adjacent conductors are in connexion with the circuit, will be expressed in the circuit itself by

$$\frac{A}{L + \Lambda};$$

in the joint conductor, whose reduced length is λ, by

$$\frac{A}{L + \Lambda} \cdot \frac{\Lambda}{\lambda};$$

in the joint conductor, whose reduced length is λ', by

$$\frac{A}{L + \Lambda} \cdot \frac{\Lambda}{\lambda'};$$

in the joint conductor, whose reduced length is λ'', by

$$\frac{A}{L + \Lambda} \cdot \frac{\Lambda}{\lambda''};$$

and so on, where for Λ its value obtained from the equation

$$\frac{1}{\Lambda} = \frac{1}{\lambda} + \frac{1}{\lambda'} + \frac{1}{\lambda''} + \&c.$$

has to be placed.

29. That in the above the galvanic current is found to be of equal magnitude at all places of the circuit, arises from the value of $\frac{du}{dx}$, deduced from the equation

$$u = \frac{A}{L} y - O + c,$$

being constant. This circumstance no longer happens if we start from the equations given in § 22 and 23. In all these cases $\frac{du}{dx}$ is dependent on x, which indicates that the magnitude of the current is different at different places of the circuit. We may hence draw the conclusion, that the electric current is only of equal intensity at all places of the circuit, when the circuit has already assumed a permanent state, and the atmosphere has no sensible action upon it. This property likewise appears best adapted to enable us to find out, by experiment, whether

the atmosphere exercises a perceptible influence on a galvanic circuit, or not, we will therefore enter into this case at greater length.

Since, according to § 12, the magnitude of the electric current is given by the equation

$$S = \varkappa\,\omega \,.\, \frac{d\,u}{d\,x},$$

we have only in each separate case to obtain the value of $\frac{d\,u}{d\,x}$ from the equation found for the determination of the electroscopic force, and to place it in the one above. Thus, for a circuit which has assumed its permanent state, but upon which the surrounding atmosphere exercises no sensible influence, according to § 22,

$$u = \tfrac{1}{2}\,a \,.\, \frac{e^{\beta x} - e^{-\beta x}}{e^{\beta l} - e^{-\beta l}} + \tfrac{1}{2}\,b\,\frac{e^{\beta x} + e^{-\beta x}}{e^{\beta l} + e^{-\beta l}},$$

where a represents the tension at the place of excitation, and b the sum of the electroscopic forces immediately adjacent on both sides of the place of excitation. We hence obtain

$$S = \varkappa\,\omega\,\beta\left(\tfrac{1}{2}\,a\,\frac{e^{\beta x} + e^{-\beta x}}{e^{\beta l} - e^{-\beta l}} + \tfrac{1}{2}\,b\,\frac{e^{\beta x} - e^{-\beta x}}{e^{\beta l} + e^{-\beta l}}\right).$$

This expression gives the magnitude of the current at each place of the circuit; but the law, according to which the alteration of the current at various places of the circuit is effected, may be rendered more easily intelligible in the following manner. If, for instance, we differentiate the equation

$$S = \varkappa\,\omega\,\frac{d\,u}{d\,x},$$

we obtain the equation

$$\frac{d\,S}{d\,x} = \varkappa\,\omega\,\frac{d^2\,u}{d\,x^2};$$

and by multiplying both together,

$$S\,\frac{d\,S}{d\,u} = \varkappa^2\,\omega^2\,\frac{d^2\,u}{d\,x^2}.$$

If we now substitute for $\frac{d^2\,u}{d\,x^2}$ its value $\beta^2\,u$, as obtained from the equation $0 = \frac{d^2\,u}{d\,x^2} - \beta^2\,u$, we have

$$S\,\frac{d\,S}{d\,u} = \varkappa^2\,\omega^2\,\beta^2\,u;$$

and we hence obtain by integration

$$S^2 = c^2 + \varkappa^2 \omega^2 \beta^2 u^2,$$

where c represents a constant remaining to be determined. If we designate by u' the smallest absolute value which u occupies in the circumference of the circuit, and by S' the corresponding value of S, and determine, in accordance with this, the constant c, we obtain

$$S^2 - S'^2 = \varkappa^2 \omega^2 \beta^2 (u^2 - u'^2).$$

It may easily be deduced from this equation, that the current of a circuit, which is influenced by the atmosphere, is weakest where the electroscopic force, without regard to the sign, is smallest, and that it is of the same magnitude at places with equal but opposite electroscopic forces.

APPENDIX.

ON THE CHEMICAL POWER OF THE GALVANIC CIRCUIT.

On the Source and character of the Chemical Changes in a Galvanic Circuit, and on the Nature of the Fluctuations of its Force dependent thereon.

30. In the present Memoir we have constantly supposed that those bodies, which are under the influence of the electric current, remain unchangeable; we will now, however, take into consideration the action of the current on the bodies subjected to it, and the alterations in their chemical constitution thence resulting in any possible manner, as also the changes of the current itself produced by reaction. If what we here give does by no means exhaust the subject, nevertheless our first attempt shows that we are advancing in this path towards important conclusions respecting the relation of electricity towards bodies.

To proceed on sure ground, let us return to what has been enounced in § 1 to 7, and connect our present considerations with those expressions and developments. We will suppose, therefore, two particles, and designate by s their mutual distance, by u and u' their electroscopic forces, which we admit to be of equal intensity in all points of the same particle; then, as may easily be deduced from what has been previously stated,

the repulsive force between these two elements is proportional to the time dt, to the product uu', and, moreover, to a function dependent on the position, size, and form of the two particles, which we will represent by F'; we accordingly obtain for the repulsive force between two particles the expression

$$F' \, u u' \, dt.$$

If we here proceed in the same manner as in § 6, and signify by the *moment of action* $\varkappa'$ between two places, the product of q', which expresses the force produced under perfectly determined circumstances between both, and its mean distance s', so that

$$\varkappa' = q' \, . \, s',$$

and determine q' by putting $u = u' = 1$ in the expression $F' \, u u' \, dt$, and extending the action to the unit of time, we have

$$\varkappa' = F' s',$$

whence it follows that

$$F' = \frac{\varkappa'}{s'}.$$

Let us now imagine, as we did in § 11, the prismatic circuit to be divided into equally large, infinitely thin discs, and call M', M, $M_{,}$ those immediately following one another, which belong to the abscissæ $x + dx$, x, $x - dx$; then, according to what has just been shown, the pressure which the disc M' exerts on the disc M is

$$F' \, u u' \, dt;$$

and if we admit that the position, size, and form of the particles remain in all discs the same, the counter pressure, which the disc $M_{,}$ exerts on the disc M, is

$$F' \, u u_{,} \, dt;$$

the difference between these two expressions, viz.

$$F' \, u \, (u' - u_{,}) \, dt,$$

gives accordingly the magnitude of the force, with which the disc M tends to move along the axis of the circuit. This force acts contrary to the direction of the abscissæ when its value is positive, and in the direction of the abscissæ when it is negative.

If we substitute for $u' - u_{,}$ its value proceeding from the developments given in § 11 for u' and $u_{,}$, the expression just found changes into the following:

$$2\,F'\,u\frac{d\,u}{d\,x}\,d\,x\;\;d\,t,$$

and if we take, instead of the function F' dependent on the nature of each single body, its value $\frac{\varkappa'}{s'}$, this expression, since s' is evidently here $d\,x$, changes into

$$2\,\varkappa'\,u\frac{d\,u}{d\,x}\,d\,t\,;$$

or if we reduce the moment of action $\varkappa'$, referring to the magnitude of the section ω, to the unit of surface, and at the same time extend the action to the unit of time, into

$$2\,\varkappa'\,\omega\,u\,\frac{d\,u}{d\,x},$$

where the present $\varkappa'$ represents the magnitude of the moment of action reduced to the unit of surface. If we write this latter expression thus:

$$2\,\frac{\varkappa'}{\varkappa}\,\varkappa\,\omega\,u\frac{d\,u}{d\,x},$$

in which $\varkappa$ denotes the absolute power of conduction of the circuit; and if we substitute for $\varkappa\,\omega\,\frac{d\,u}{d\,x}$, by which, according to the equation (b) in § 12, the magnitude of the electric current is expressed, the sign S chosen for it, and i instead of $\frac{\varkappa'}{\varkappa}$, it is changed into

$$2\,i\,u\,S.$$

We hence perceive that the force, with which the individual discs in the circuit tend to move, is proportional, both to their innate electroscopic force, and to the magnitude of the current; and that this force alters its direction at that place of the circuit where the electricity passes from the one into the opposite state. And here occurs the circumstance which must not be overlooked, that this expression still holds, even when the electroscopic force u of the element M is changed in the moment of action, by any causes whatsoever, into any other abnormal U, while the electroscopic forces of the adjacent particles continue the same; only that in this case the value U must be substituted for u in the expression $2\,i\,u\,S$. It must also be observed, that the expression $2\,i\,u\,S$ which we have found refers to the whole extent of the section ω, which belongs to that part of

the circuit which we have especially in view; if we wish to reduce this motive force of the circuit to the unit of surface, we must divide that expression by the magnitude of the section ω.

With respect to the causal relation between the law of electric attractions and repulsions, and that of the diffusion of electricity, or respecting the mutual dependence of the functions $\varkappa$ and $\varkappa'$ on each other, we will, for the present, not enter into any further inquiries, as shortly an occasion will present itself for this purpose. We will here content ourselves with the observation, that the above mode of explanation has arisen from the endeavour to render the similarity of the mode of treatment in the doctrines of electricity and heat very obvious.

31. Without pursuing any further these conditions to an external change of place of the parts of a galvanic circuit, let us now turn to those changes in the qualitative state of the circuit which are produced by the electric current, *i. e.* in the internal relation of the parts to each other, and which derive their explanation from the electro-chemical theory of bodies. According to this theory, compound bodies must be considered as a union of constituents which possess dissimilar electric states; or, in other words, dissimilar electroscopic force. But this electroscopic force, quiescent in the constituents of the bodies, differs from that to which our attention has hitherto been directed, inasmuch as it is linked to the nature of the elements, and cannot pass from one to the other, without the entire mode of existence of the parts of the body being destroyed. If we confine ourselves, therefore, in the following considerations, to the case where changes, it is true, occur in the quantitative relation of the constituents, and where consequently chemical changes of the body, composed of these constituents, also occur, but where the constituents themselves undergo no alteration destroying their nature, we are able to show the validity of all the laws above developed of electric bodies with reference to their reciprocal attraction and repulsion, only the transition of the electricity from one particle to the other entirely disappears in the consideration of chemically different constituents. A distinction here exists with reference to electricity exactly similar to that which we are accustomed to define relative to heat, by calling it sometimes latent, sometimes free heat. For the sake of brevity, we will in like manner term that electroscopic force

which belongs to the existence of the particles, which therefore they cannot part with without at the same time ceasing to exist, the *electricity bound* to the bodies, or *latent electricity*, and *free electricity*, that which is not requisite for the existence of the bodies in their individuality, and which therefore can pass from one element to the other, without the individual parts being on that account compelled to exchange their specific mode of existence for another.

32. From these suppositions advanced in electro-chemistry, in connexion with what was stated in § 30, respecting the mode in which galvanic circuits exert a different mechanical force on discs of different electrical nature, it immediately follows that when a disc belonging to the circuit is composed of constituents of dissimilar electric value, the neighbouring discs will exert on these two constituents a dissimilar attractive or repulsive action, which will excite in them a tendency to separate, which, when it is able to overcome their coherence, must produce an actual separation of constituents. This power of the galvanic circuit, with which it tends to decompose the particles into their constituents, we will call its *decomposing force*, and now proceed to determine more minutely the magnitude of this force.

Employing for this purpose all the signs introduced in § 30, we will, moreover, imagine each disc to be composed of two constituents, A and B, and designate by m and n the latent electroscopic forces of the constituents A and B, supposing the disc M to be occupied solely by one of the two, entirely excluding the other, in the same manner as u represents the free electroscopic force present in the same disc, and equally diffused over both constituents. If we now admit, in order to simplify the calculation, that the two constituents A and B, before and after their union, constantly occupy the same space, and designate the latent electroscopic force, corresponding to each chemical equivalent, contained in the disc M, and proceeding from the constituent A, by $m z$, then $n (1-z)$ expresses the latent electroscopic force present in the same disc M, but originating from the constituent B: for the intensity of the force diffused over a body decreases in the same proportion as the space which the body occupies becomes greater, because by the increased distance of the particles from each other the sum of their actions, restricted to a definite extent, is diminished in

the same proportion. But when two constituents combine, by both reciprocally penetrating one another, each extends beyond the entire space of the compound, on which account the intensity of the force proper to each constituent decreases by combination, in the same proportion as the space of the compound is greater than the space which each constituent occupied before the combination. Consequently if z denote the relation of the space which the constituent A, in the disc M, occupied previous to combination to that space which the compound in the disc M occupies; and also, since we admit that both constituents, before and after the combination, occupy the same extent of space, $1-z$ will denote the same relation relatively to the constituent B; then, since m and n designate the electroscopic forces of the constituents A and B previous to combination, $m\,z$ and $n\,(1-z)$ will represent the latent electroscopic forces of the constituents A and B, which correspond to each chemical equivalent of the disc M; and, at the same time, it follows from the above, that the variable values z and $1-z$ cannot exceed the limits 0 and 1.

In order to ascertain the portion of the free electricity u pertaining to each constituent, we will assume that it is distributed over the single constituents in proportion to their masses. If, therefore, we represent respectively by α and β the masses of the constituents A and B, on the supposition that one alone, to the exclusion of the other, occupies the entire disc, then $\alpha\,z$ and $\beta\,(1-z)$ will represent the masses of the constituents A and B united in the disc M; consequently the portions

$$\frac{\alpha\,u\,z}{\alpha\,z+\beta(1-z)}, \text{ and } \frac{\beta\,u\,(1-z)}{\alpha\,z+\beta\,(1-z)}$$

of the free electricity u appertain to the constituents A and B; instead of which, for the sake of conciseness, we will write

$$\alpha\,\mathrm{U}\,z, \text{ and } \beta\,\mathrm{U}\,(1-z).$$

If we now take into consideration what was stated in § 30, respecting the motive force of the galvanic circuit, it is immediately evident that the tendency of the constituent A to move along the circuit, is expressed by

$$2\,i\,(m+\alpha\,\mathrm{U})\,z\,\mathrm{S},$$

or that of the constituent B by

$$2\,i\,(n+\beta\,\mathrm{U})\,(1-z)\,\mathrm{S}.$$

In both cases a positive value of the expression shows that the pressure takes place in an opposite direction to that of the abscissæ; a negative value, on the contrary, indicates that the pressure is exerted in the direction of the abscissæ. To deduce from these individual tendencies of the constituents the force with which both endeavour to separate from each other, we must remember that this force is given by the twofold difference between the quantities of motion which each constituent would of itself assume, were it associated to the other by no coherence, and those quantities of motion which each constituent must assume were it strongly combined to the other. We thus readily find for the decomposing force of the circuit the following expression:

$$4\,i\,.\,z\,(1-z)\,.\,\frac{m\,\beta-n\,\alpha}{\alpha\,z+\beta\,(1-z)}\,.\,\mathrm{S},$$

from which we learn that the decomposing force of the circuit is proportional to the electric current, and also to a coefficient dependent on the chemical nature of each place of the circuit.

If this expression has a positive value, it indicates that the separation of the constituent A takes place in a contrary direction to that of the abscissæ, that of the constituent B in the direction of the abscissæ; but if this expression has a negative value, it denotes a separation in the reverse direction. It is besides evident, at first sight, that the decomposing force of the circuit is constantly determined by the absolute value of the expression.

If $\alpha=\beta$, the decomposing force of the circuit changes into

$$4\,i\,.\,z\,(1-z)\,(m-n)\,.\,\mathrm{S}.$$

If $m\,z+n\,(1-z)=0$, *i. e.* if the latent electroscopic forces, existing in the united constituents, are equal and opposed; or, what is the same, if the body, situated in the disc M, is perfectly neutral, in which case m and n have constantly opposite values, we obtain, for the decomposing force of the circuit, the following expression:

$$4\,i\,.\,\frac{m\,n}{m-n}\,.\,\mathrm{S}.$$

The form of the general expression found for the decomposing force of the circuit shows that this force disappears; first, when $\mathrm{S}=0$, *i. e.* when no electric current exists; secondly,

when $z=0$, or $z=1$, *i. e.* when the body to be decomposed is not compound; thirdly, when $m\beta-nz=0$, *i. e.* when the densities of the constituents are proportional to the latent electroscopic forces which they possess, which circumstance can never occur with constituents of opposite electric nature.

All the expressions here given for the decomposing force of the circuit refer to the entire section belonging to the respective place; if we wish to reduce the value of the decomposing force to the unity of surface, the expression must be divided by the magnitude of the section, to which attention has been already called in § 30, in a similar example.

33. If this decomposing force of the circuit is able to overcome the coherence of the particles in the disc, a coherence produced by their electric opposition, this necessarily occasions a change in the chemical equivalent of the particles. But such a change in the physical constitution of the circuit must, at the same time, react on the electric current itself, and give rise to alterations in it, with which a more accurate acquaintance is desirable, and which we will therefore spare no trouble to acquire.

For this purpose we will imagine a portion of the galvanic circuit to be a homogeneous fluid body, in which such a decomposition actually takes place; then, at all points of this portion, the elements of one kind will tend to move with greater force towards one side of the circuit than those of the other kind; and since we suppose that, by the active forces, the coherence is overcome, it follows, if we pay due attention to the nature of fluid bodies, that the one constituent must pass to one side, those of the other constituent, on the contrary, towards the other side of the portion, which necessarily produces on one side a preponderance of the constituent of one kind, and on the other side a preponderance of the other kind of constituent. But as soon as a constituent is predominant on one side of any disc, it will oppose by its preponderance the movement of the like constituent in the disc towards the same side, in consequence of the repulsive force existing between both; the decomposing force, therefore, has now not merely to overcome the coherence between the two constituents in the disc, but also the reacting force in the neighbouring discs. Two cases may now occur; the decomposing force of the electric current either constantly overcomes all the forces opposed to it,

and then evidently the action terminates by a total separation of the constituents, the entire mass of the one passing to the one end of the portion, and the entire mass of the other constituent being impelled towards the other end of this portion; or such a relation takes place between the forces in action, that the forces opposing the separation ultimately maintain the decomposing force in equilibrium; from this moment no further decomposition will occur, and the portion will be, in a remarkable state, a peculiar distribution of the two constituents occurring, into the nature of which we will now inquire. If we call Z the decomposing force of the current in any disc of the portion in the act of decomposition, Y the magnitude of the reaction by which the neighbouring discs oppose the decomposition by the electric current, and X the force of the coherence of the two constituents in the same disc, then evidently the state of a permanent distribution within the supposed portion, will be determined by the equation

$$X + Y = Z;$$

and it is already known, from the preceding paragraph, that

$$Z = 4\, i\, z\, (1 - z)\, \frac{m\,\beta - n\,\alpha}{\alpha\, z + \beta\, (1 - z)}\,.\, S;$$

or if we substitute $\varkappa\,\omega\,\frac{d\,u}{d\,x}$ for S,

$$Z = 4\,\varkappa\,\omega\,\frac{d\,u}{d\,x}\,.\, i\, z\, (1 - z)\, \frac{m\,\beta - n\,\alpha}{\alpha\, z + \beta\, (1 - z)}.$$

Before we proceed further, we will add to what has been above said the following remarks. At the limits of the portion in question, we imagine the circuit so constituted, that insuperable difficulties there oppose themselves to any further motion; for it is obvious that otherwise the two extreme strata of both constituents, which it is evident could never of themselves arrive at equilibrium, would quit the portion in which we have hitherto supposed them, and either pass on to the adjacent parts of the circuit, or from any other causes separate entirely from the circuit. We will not here follow the last-mentioned modification of the phænomenon any further, although it frequently occurs in nature, as sufficiently shown by the decomposition of water, the oxidation of the metals on the one side, and a chemical change of a contrary kind occurring on the metals at the other side of the portion hitherto less ob-

served, but placed entirely beyond doubt by *Pohl's* remarkable experiments on the reaction of metals. Besides, we will direct our attention to a difference which exists between the distributioh of electricity above examined, and the molecular movement now under consideration. If, for instance, the same forces, which previously effected the conduction of the electricity, and there, as it were, incorporeally without impediment strove against each other, here enter into conflict with masses, by which their free activity is restricted, a restriction which, whether we regard the electricity *de se ipso* as something material or not, must render their present velocities, beyond comparison, smaller than the former ones; therefore we cannot in the least expect that the permanent state, which we at present examine, will instantaneously occur like that above noticed, arising from the electric distribution; we have rather to expect that the permanent state resulting from the chemical equivalent of both constituents, will make its appearance only after a perceptible, although longer or shorter time.

After these remarks, we will now proceed to the determination of the separate values X and Y.

34. To obtain the value X, we have merely to bear in mind that the intensity of coherence is determined by the force with which the two adjacent constituents attract or repel each other by virtue of their electric antagonism, and consequently, as was shown in § 30, proportional to the product of the latent electroscopic forces $m z$ and $n(1-z)$ possessed by the constituents of the disc M, and is, moreover, dependent on a function to be deduced from the size, form, and distance, which we will designate by 4ϕ. Accordingly, when we restrict the coherence to the magnitude of the section ω,

$$X = -4\phi\, m n z (1-z)\, \omega.$$

We have placed the sign — before the expression ascertained for the strength of the coherence, since a reciprocal attraction of the constituents only occurs when m and n have opposite signs; when m and n have the same signs, the constituents exert a repulsive action on each other, which no longer prevents, but promotes the decomposing force. After this remark it will at first sight be evident that a positive or negative value must be ascribed to the function ϕ, according as the expression taken for the decomposing force z is positive or nega-

tive; the sign of the function ϕ, therefore, changes when the direction of the decomposition is transposed from the one constituent to the other. The nature of the function ϕ is as little known to us as the size and form of the elements on which it is dependent; nevertheless, we may, in our inquiries, regard its absolute value as constant, since the size and form of the corporeal particles, acting on each other, must be conceived to be unchangeable so long as the two constituents remain the same, and the supposition that the two constituents constantly maintain for every chemical equivalent the same volume, renders attention to the mutual distance of the chemically different particles unnecessary, as regard has already been paid, when determining the electroscopic forces in the disc M, to the relative distances of the elements of each constituent.

35. To determine the magnitude of the reaction Y, which in the disc M opposes the latent electricity of the neighbouring discs to the decomposing force, we have nothing further to do than to substitute in the expression for $\mathfrak{Z}$ instead of u, the sum of all the latent electroscopic forces in the disc M. Since now the sum of these latent forces is $m z + n (1 - z)$, we obtain for the determination of the force Y, which is called into existence by the change in the chemical equivalent of the constituents, and which opposes the decomposition, after due determination of its sign, the following equation:

$$Y = 4 \varkappa \omega \frac{d z}{d x} \cdot i (n - m) \cdot z (1 - z) \cdot \frac{m \beta - n \alpha}{\alpha z + \beta (1 - z)}.$$

If now we substitute for $\mathfrak{X}$, $\mathfrak{Y}$, and $\mathfrak{Z}$ the values found in the equation

$$X + Y = Z,$$

we obtain, after omitting the common factor $4 z (1 - z)$, and multiplying the equation by $\dfrac{\alpha z + \beta (1 - z)}{i (m \beta - n \alpha)}$, as the condition of the permanent state in the chemical equivalent of the two constituents, the equation

$$0 = \varkappa \omega \frac{d u}{d x} + \frac{\phi m n}{i (m \beta - n \alpha)} \times \cdot [\alpha z + \beta (1 - z)] \omega$$
$$- \varkappa \omega (n - m) \frac{d z}{d x},$$

which, when we put

$$\frac{\phi\, m n}{i(m\,\beta - n\,\alpha)} = \psi = \frac{\varkappa\,\phi\, m n}{\varkappa'\,(m\,\beta - n\,\alpha)},$$

passes into

$$0 = \varkappa\,\omega\,\frac{d\,u}{d\,x} + \psi\,\omega\,[\alpha\,z + \beta\,(1 - z)] - \varkappa\,\omega\,(n - m)\,\frac{d\,z}{d\,x}. \quad (\text{ठ})$$

This equation undergoes no change, as indeed is required by the nature of the subject, when m, α, z, and n, β, $1 - z$ are respectively interchanged, and, at the same time, the sign of ϕ is changed, as according to the remark made in the preceding paragraph, must take place, since by this transformation the direction of the decomposition is transferred from one constituent to the other.

36. In order to be able to deduce from this equation the mode of the diffusion of the two constituents in the fluid, *i. e.* the value of z, we ought to know the power of conduction $\varkappa$, and the electroscopic force u at each point of the portion in the act of decomposition, the values, however, of which, are themselves dependent on that diffusion. Experience, as yet, leaves us in uncertainty respecting the change of conductibility, which occurs when two fluids are mixed in various proportions with one another, and likewise with respect to the law of tensions, which is followed by different mixtures of the same constituents in various proportion; for, if we do not err, no experiments have been instituted relatively to the latter law, and the law of the change produced in the conducting power of a fluid, by the mixture of another, is not yet decidedly established by the experiments of Gay Lussac and Davy. For this reason we have been inclined to supply this want of experience by hypothesis. We have, it is true, constantly endeavoured to conceive the nature of the action in question, in its connexion with those with whose properties we are better acquainted; but, nevertheless, we wish the determinations given to be regarded as nothing more than fictions, which are only to remain until we become by experiment in possession of the true law.

With regard to what relates to the change in the power of conduction of a body, by mixture with another, we have been guided by the following considerations. We suppose two adjacent parts of a circuit of the same section ω, whose lengths are v and w, and whose powers of conduction are a and b; then, when A is the sum of the tensions in the circuit, and L the reduced length of the remaining portion of the circuit, the mag-

nitude of its current, which results from the above-found formulæ, is

$$\frac{A}{L + \frac{v}{a\,\omega} + \frac{w}{b\,\omega}}.$$

If now a conductor of the length $v + w$, and of the power of conduction $\varkappa$ with the same section, being taken instead of the two former, leaves the current of the circuit unchanged, then must

$$\frac{v}{a\,\omega} + \frac{w}{b\,\omega} = \frac{v+w}{\varkappa\,\omega},$$

whence we find

$$\varkappa = \frac{a\,b\,(v+w)}{b\,v + a\,w}.$$

But it is perfectly indifferent for the magnitude of the current, whether the entire length v be situated near the entire length w, or any number of discs be formed of the two, which are arranged in any chosen order, if only the extreme parts remain of the same kind, as otherwise a change might result in the sum of the tensions, consequently also in the magnitude of the current. If we extend this law, which holds for every mechanical mixture, likewise to a chemical compound, the above value found for $\varkappa$ evidently gives the conducting power of the compound, where, however, it has been taken for granted that the two parts of the circuit, even after the mixture, still occupy the same volume, for v and w are here evidently proportional to the spaces occupied by the two bodies mixed with each other.

If we now apply this result to our subject, and therefore put, instead of v and w, the values z and $1 - z$, which express the relations of space of the two constituents in the disc M, we obtain, when a denotes the conducting power of the one constituent A, and b the same for the constituent B; further, $\varkappa$ the power of conduction of the mixture of the two contained in the disc M, the following expression for $\varkappa$,

$$\varkappa = \frac{a\,b}{a + (b-a)\,z}.$$

37. Having thus determined the power of conduction at each place of the extent in the act of decomposition, there only remains to be ascertained the nature of the function u at each such place; and since all tensions and reduced lengths in the

part of the circuit, in which no chemical change occurs, are unalterable and given, it is, in accordance with the general equation given in § 18, which likewise holds for our present case, only requisite for the perfect knowledge of the function u, that we are able to determine the tensions and reduced lengths for each place within the extent in which the chemical change takes place.

But evidently the reduced length of the disc M is

$$\frac{dx}{\varkappa\,\omega};$$

or if we substitute for $\varkappa$ its value just found,

$$\frac{a+(b-a)\,z}{a\,b\,\omega}\,dx;$$

we accordingly obtain the reduced length of any part of that extent, if we integrate the above expression, and take the limits of the integral corresponding to the commencement and end of the part. If now we bear in mind that the integral

$$\int \frac{a+(b-a)\,z}{a\,b\,\omega}\,dx$$

may also be written thus:

$$\frac{l}{b\,\omega}+\frac{b-a}{a\,b\,\omega^2}\int z\,\omega\,dx,$$

when l represents the length of the part, over which the integral is to be extended, and $z\,\omega\,dx$ expresses merely the space which the constituent A in the disc M occupies; consequently $\int z\,\omega\,dx$, the sum of all the spaces which the constituent A fills in the part whose reduced length has to be found, it is obvious that the reduced length of the entire portion, in the act of decomposition, remains unchangeable during the chemical change, since, as we have supposed, each constituent maintains, under all circumstances, constantly the same volume. The same result may also be directly deduced from what was advanced in the preceding paragraph; however, this unchangeability only relates to the reduced length of the *entire* portion; the reduced length of a *part* of it does not in general depend merely on the actual length of this part, but likewise on the contemporaneous chemical distribution of the constituents in the extent, and must therefore, in each separate case, be first ascertained in the manner indicated.

38. We have lastly to determine the alteration in the tension of the circuit, which is produced by the chemical alteration of the extent, which has hitherto been considered. For this purpose we assume, till experience shall have taught us better, the position, that the magnitude of the electric tension between two bodies is proportional, first to the difference of their latent electroscopic forces, and secondly to a function, which we will term the *coefficient of the tension*, dependent on the size, position and form of the particles which act on each other at the place of contact. Not only from this hypothesis may be deduced the law which the tensions of the metals observe *inter se*,—nothing further being requisite than to assume the same coefficient of tension between all metals placed under similar circumstances,—but it likewise affords an explanation of the phænomenon, in accordance with which the electric tension does not merely depend on the chemical antagonism of the two bodies, but also on their relative density, and can for this reason exhibit themselves differently, even in different temperatures. For the same reasons which we have already mentioned in § 34 on the determination of the coherence which occurs between the two constituents of a mixed body, we shall likewise admit here, in the circumference of the chemically variable extent as constant, the unknown function dependent on the size, form and position of the particles in contact, and designate it by ϕ'. Since now the latent electroscopic force in the disc M, to which the abscissa x belongs, is expressed by

$$n + (m - n)\, z,$$

and that in the disc M′, to which the abscissa $x + dx$ belongs, by

$$n + (m - n)\, z + (m - n)\, dz,$$

the tension originating between the discs M and M′ is

$$-\phi'\, (m - n)\, dz;$$

consequently the sum of all the tensions produced throughout a portion exposed to chemical change

$$-\phi'\, (m - n)\, (z'' - z'),$$

when z' and z'' represent those values of z, which belong to the commencement and end of the extent in question.

But the tension of the circuit undergoes, besides the change just explained, a second one, from the extremities of the che-

mically changeable portion, which are in connexion with the other chemically unchangeable parts of the circuit, undergoing a gradual change during the decomposition till they arrive at their permanent state, giving rise at those places to an altered tension. If, for instance, we call ζ the value of z, which belongs to all places of the extent in question, before chemical change has begun in it, and designate the coefficient of the tension occurring at the extremities of this extent, supposing that it is the same at both ends, by ϕ'', and moreover express by μ and ν the latent electroscopic forces of those places of the chemically unalterable part of the circuit which are situated adjacent to the chemically changeable extent, the tensions existing at these places can be determined individually. They are, namely, previous to the commencement of chemical change, the following:

$$\phi'' \left[\mu - (n + (m-n)\,\zeta)\right], \text{ and}$$
$$\phi'' \left[(n + (m-n)\,\zeta) - \nu\right];$$

and after the permanent state in the decomposition has been attained, if we, as above, let z' and z'' be those values of z which belong in this state to those places, they are the following:

$$\phi'' \left[\mu - (n + (m-n)\,z')\right], \text{ and}$$
$$\phi'' \left[(n + (m-n)\,z'') - \nu\right],$$

their sum is therefore in one case

$$\phi''\,(\mu - \nu),$$

and in the other

$$\phi''\,(\mu - \nu) + \phi''\,(m-n)\,(z'' - z');$$

consequently the increase of tension at those places is

$$\phi''\,(m-n)\,(z'' - z').$$

If we add this change of the tension to that above found, we obtain for the entire difference of the tension, produced by the decomposition until the commencement of the permanent state,

$$(\phi'' - \phi')\,(m-n)\,(z'' - z'),$$

which, if we substitute Φ for $\phi'' - \phi'$, changes into

$$\Phi\,(n-m)\,(z'' - z').$$

If now we represent by S the magnitude of the current, and by A the sum of the tensions in the circuit, before any chemical change has commenced, by S′ the magnitude of the current, after the permanent state has been attained; lastly, by L the

reduced length of the entire circuit, which, as we have seen, remains under all circumstances the same, it results

$$S' = \frac{A - \Phi\,(n-m)\,(z''-z')}{L};$$

or, if we write for $\frac{A}{L}$ its equivalent S,

$$S' = S - \frac{\Phi\,(n-m)\,(z''-z')}{L},$$

so that, therefore, $\frac{\Phi\,(n-m)\,(z''-z')}{L}$ designates the decrease produced in the magnitude of the current by the chemical alteration.

39. After all these intermediate considerations, we now proceed to the final determination of the chemical alteration in the changeable portion, and the change of the current in the whole circuit produced by this chemical alteration, where, however, we have constantly to keep in view only the permanent state of the altered portion. If we substitute in the equation ($\mathfrak{z}$) given in § 35, for $\varkappa\,\omega\,\frac{d\,u}{d\,x}$ its value S′, which, as we have just found, is solely dependent on the fixed and unalterable values of z, and therefore has to be treated in the calculation as a constant magnitude; further, for $\varkappa$ its value $\frac{a\,b}{a+(b-a)\,z}$, given in § 36, this equation changes into

$$0 = S' + \psi\,\omega\,\beta + \psi\,\omega\,(\alpha-\beta)\,z - \frac{a\,b\,\omega\,(n-m)}{a+(b-a)\,z}\cdot\frac{d\,z}{d\,x};$$

or if we place $S' + \psi\,\omega\,\beta = \Sigma$, and $\psi\,\omega\,(\alpha-\beta) = \Omega$, into

$$0 = \Sigma + \Omega\,z - \frac{a\,b\,\omega\,(n-m)}{a+(b-a)\,z}\cdot\frac{d\,z}{d\,x},$$

from which, by integration, we deduce the following:

$$c = \frac{(b-a)\,\Sigma - a\,\Omega}{a\,b\,\omega\,(n-m)}\,x + \log\frac{\Sigma+\Omega\,z}{a+(b-a)\,z},$$

where c represents a constant remaining to be determined. If we designate by χ the abscissa of that place of the chemically changed portion for which z has still the same value, which, previous to the commencement of the chemical decomposition, belonged to each place of this portion, for which therefore $z=\zeta$,

and determine in accordance with this statement the constant c, our last equation acquires the following form:—

$$\frac{\Sigma + \Omega z}{a + (b-a) z} = \frac{\Sigma + \Omega \zeta}{a + (b-a)\zeta} \cdot e^{\frac{(b-a)\Sigma - a\Omega}{a b \omega (n-m)}(\chi - x_{,})}$$

where e denotes the base of the natural logarithms. The following consideration leads to the determination of the value χ. Since, namely, ζ represents the space which the constituent A occupies in each individual disc of the changeable portion previous to the commencement of the chemical decomposition, if we denote by l the actual length of this portion, $l\zeta$ expresses the sum of all the spaces which the constituent A occupies on the entire expanse of the changeable portion; but this sum must constantly remain the same, since, according to our supposition, no part of either of the constituents is removed from this portion, and both maintain, under all circumstances, the same volume, even after chemical decomposition has taken place; we obtain, therefore,

$$l\zeta = \int z\, dx,$$

where for z is to be substituted its value resulting from the previous equation, and the abscissæ corresponding to the commencement and end of the changeable portion are to be taken as limits of the integral.

These two last equations, in combination with that found at the end of the previous paragraph, answer all questions that can be brought forward respecting the permanent state of the chemical alteration, and the change in the electric current thus produced, and so form the complete base to a theory of these phænomena, the completing of the structure merely awaiting a new supply of materials from experiment.

40. At the conclusion of these investigations we will bring prominently forward a particular case, which leads to expressions that, on account of their simplicity, allow us to see more conveniently the nature of the changes of the current produced by the chemical alteration of the circuit. If, for instance, we admit $a = b$, and $\alpha = \beta$, the differential equation obtained in the preceding paragraph changes into the following:

$$0 = \Sigma\, dx - a\omega(n-m)\, dz,$$

whence we obtain by integration

$$z-\zeta = \frac{\Sigma\,(x-\chi)}{a\,\omega\,(n-m)},$$

when χ designates the value of x, for which $z=\zeta$. Since in this case the value of z constantly changes to the same amount on like differences of the abscissæ, the abscissa χ, which belongs to its mean value ζ, as it was at all places of the changeable portion previous to the commencement of the chemical decomposition, must be referred to the middle of this portion. If, therefore, z' and z'', as above, represent the values of z, which correspond to the commencement and end of the variable portion, and l the actual length of this portion, it follows, from our last equation, that

$$z''-\zeta = +\tfrac{1}{2}\,\frac{l\,\Sigma}{a\,\omega\,(n-m)},$$

and

$$z'-\zeta = -\tfrac{1}{2}\,\frac{l\,\Sigma}{a\,\omega\,(n-m)},$$

and from these two equations results

$$(n-m)\,(z''-z') = \frac{l}{a\omega}\,.\,\Sigma\,;$$

or, if we put, instead of $\frac{l}{a\,\omega}$, by which here nothing further is expressed than the unchangeable reduced length of the chemically variable portion, the letter λ, the following:

$$(n-m)\,(z''-z') = \lambda\,\Sigma.$$

If we place this value of $(n-m)\,(z''-z')$ in the equation found in § 38,

$$S' = S - \frac{\Phi\,(n-m)\,(z''-z')}{L},$$

and at the same time substitute for Σ its value $S'+\psi\,\omega\,\alpha$, we obtain

$$S' = S - \frac{\Phi\,\lambda}{L}\,(S'+\psi\,\omega\,\alpha),$$

an equation, the form of which is extremely well suited to indicate in general the nature of the change of the current produced by the chemical alteration, and the expressions of which coincide exceedingly well with the numerous experiments I have made on the fluctuation of the force in the hydro-circuit, and of which only a small part have been published*.

* Schweigger's *Jahrbuch*, 1825, Part 1 and 1826, Part 2.

Article XIV.

Selections from a Memoir on the Expansion of Dry Air. By the late Professor F. Rudberg.

[From Poggendorff's *Annalen*, B. 41. S. 271.]

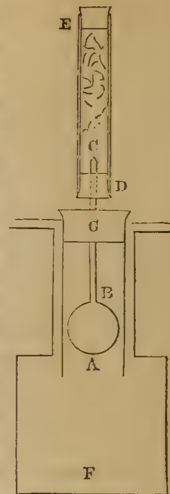
AMONG the constants in physics there is certainly not one which is usually considered to be determined with greater precision than the expansion of dry air, or of dry gases generally, under a constant pressure, between the standard points of the thermometer scale. The numerous experiments made by Dalton and Gay Lussac, almost at the same time, about the beginning of the present century, appeared to show, beyond all doubt, that the amount of this expansion from 0° to 100° C., under a constant pressure, was 0·375 of the volume of the air at 0°. Their great skill in experimenting, and the magnitude and number of the services they had rendered science, left no room for any doubt as to the accuracy of this result; consequently, for more than thirty years in all computations in which the expansion of gas occurs, it has been assumed to be 0·375.

The constant in question is undeniably of the greatest importance in Physics, since it forms the basis of all methods of measuring temperature; it is used in the explanation of most of the phænomena caused by heat; and lastly, is requisite in the reduction of many observations in Physics and other sciences; as, for example, in determining the velocity of sound, in the measurement of heights by means of the barometer, and in computing astronomical refractions. This being the case, it will no doubt appear surprising, that the value of this constant, which has been employed up to the present time, is erroneous to no small amount, since, as will be shown in this memoir, it appears to be not more than from 0·364 to 0·365, instead of 0·375.

The change of volume produced by heat can be determined, either by heating cold air and measuring the increase of its volume, or by cooling warm air and determining the diminution of its volume. I have adopted the latter method, as being by far the most accurate.

In most of the experiments, a glass globe, having a neck made of thermometer tube A B C (fig. 1.), and capable of con-

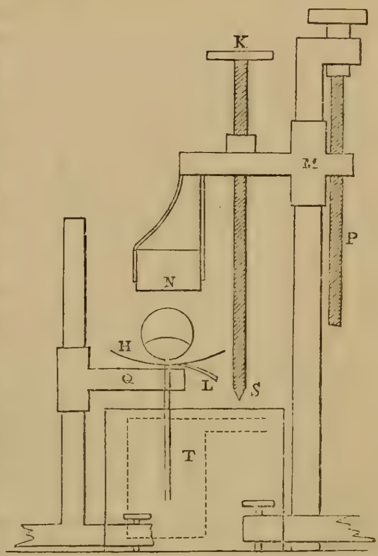
taining from 120 to 150 grammes of mercury, was used for containing the air. After the end of the tube had been fitted into a hole in a cork at one end of a cylinder, D E, containing chloride of calcium, the air was dried, either by heating the globe strongly over a spirit-lamp, and then suffering it to cool, and repeating the process at least fifty or sixty times; or else by connecting the end E of the cylinder with an air-pump, and exhausting and re-admitting the air fifty or sixty times. I have not observed any difference between these two methods of drying air, but have found one as effectual as the other. The chloride of calcium was fused at a red heat, then poured out upon a cold plate of metal, and as soon as it became solid, broken to pieces, and put into bottles with ground stoppers while red hot.



The globe having been dried in this manner, and remaining in connexion with the chloride of calcium tube, a small opening being made in the cork at E for the air to escape through, was suspended by means of a cork G cut in two, in the boiler F, the upper part of which, as described in my memoir on the construction of thermometers (Poggendorff's *Annalen*, B. 40.), consists of two concentric cylinders, so that the globe and the greater part of the tube were surrounded by steam. After the water had been boiling three quarters of an hour, or an hour, the cylinder D E was removed, and the boiling continued for about ten minutes longer. The height of the mercury in the barometer was then observed, and the tube sealed, the water being kept boiling freely in the mean while.

After the ball had been weighed with a balance, which turned with one-tenth of a milligramme, it was firmly fixed to the arm Q (fig. 2.) of a steady support, with the tube passing through a hole, in a metal dish H. The arm Q was then so far depressed, that the point of the tube was deeply immersed in the mercury of the trough T. Lastly, the point of the tube was broken off, and in order that all the mercury requisite might enter, the ball was suffered to remain in this situation several hours, almost always all night, although I had convinced myself that not more than a quarter of an hour

at most was requisite, with even the smallest of the tubes which I employed.



Snow was now placed on the metal dish H, and the globe surrounded with it on all sides. The water produced by the melting of the snow escaped through the tube L. As soon as the snow began to melt, fresh snow was carefully added, so that the temperature of the globe was kept at 0° for about two hours, and sometimes even longer. When by this means I was certain that all the mercury had actually entered which at 0° could be forced in by the pressure of the atmosphere, I closed the fine opening of the tube with a very soft mixture of wax and turpentine, which was prepared for that purpose in a little spoon of iron. At the same instant the barometer was observed, in order to determine the existing pressure of the atmosphere; the snow was then carefully removed, and the difference of altitude between the surfaces of the mercury within and without the globe measured.

For this purpose the measuring apparatus N M K was prepared. Upon the whole, it depends upon the principle employed in measuring the height of the mercury in Fortin's barometers. A slider M, embracing tightly the vertical bar, is moved up or down by a screw P, and therefore also the cylindrical ring N, and screw K, which are connected with it. The ring N having first been made accurately horizontal, was depressed, surrounding the globe, till its under edge coincided with the surface of the mercury in the globe, and the screw K S turned till its point S just touched the surface of the mercury in the trough. It is evident that the difference of altitude between the under edge of the ring and the point S was equal to the difference of altitude of the two surfaces of the mercury. After the contacts had

been made as accurately as possible, the measuring apparatus was removed, and the globe, the extremity of the tube being closed with wax, as has been already stated, lifted out of the trough. The difference of altitude of N and S was then accurately measured, by means of two graduated scales, placed at right angles to each other, and the globe, with the mercury which had been forced into it, weighed after the wax had been removed.

When this was accomplished, the tube was bent at the end, so that it could be dipped into a vessel of mercury, the globe filled with it, and all the air expelled, by carefully boiling. When cold it was placed in snow, and completely filled with mercury at 0°. When no more mercury could be introduced (this was known to be the case by the thread of mercury showing itself at the extremity of the tube), a clean empty vessel was placed underneath, to receive the mercury that ran out; the globe taken out, and placed in the boiler. The mercury that escaped between the temperatures of 0° and temperature of boiling determined by the height of the barometric column, was weighed, and the weight of this, added to the weight of the mercury remaining in the globe, consequently gave the weight of mercury contained in the globe at 0°. From these two weights, and the true expansion of mercury, the true expansion of the glass may be calculated.

Let u be the volume of the globe at 0°, and therefore the volume of the mercury contained in it at that temperature; h the height of the barometric column, in centimetres, at the instant the tube was sealed; T the corresponding temperature of the vapour of boiling water; 100 A the true expansion of dry air from 0° to 100°; 100 G the expansion of glass in volume from 0° to 100°. At the instant the end of the tube was closed with wax, let v be the volume of the air contained in it; q the weight of the mercury contained in it; k the height of the mercury in the barometer; l the height of the surface of the mercury in the globe above the surface of the mercury in the trough; p the weight of the mercury contained in the globe at 0°. The volumes, pressures, and temperatures of the air at the time the tube was sealed, and at the time it was closed with wax, were as $u\,(1 + \mathrm{G\,T})$, v; h, $k - l$; T°, 0° respectively, therefore

$$\frac{u\,(1 + \mathrm{G\,T})}{v} = \frac{k - l}{h}\,(1 + \mathrm{A\,T}).$$

But

$$\frac{v}{v} = \frac{p}{p - q},$$

Therefore

$$\frac{p\,(1 + \mathrm{G\,T})}{p - q} = \frac{k - l}{h}\,(1 + \mathrm{A\,T}).$$

Let r' be the weight of the mercury expelled from the globe when heated from 0° to T'°; r the weight of the mercury expelled when heated from 0° to 100°; 100 M the true expansion of mercury from 0° to 100°; b, b' the weights of a unit of volume of mercury at 0° and 100° respectively. Then

$$\frac{r}{100} = \frac{r'}{\mathrm{T}'};$$

$$b'\,(1 + 100\,\mathrm{M}) = b;$$

the volume of the mercury at 100° $= u\,(1 + 100\,\mathrm{M})$; the volume of the globe at 100 $= u\,(1 + 100\,\mathrm{G})$; therefore the volume of the mercury at 100° expelled $= u\,.\,100\,(\mathrm{M} - \mathrm{G})$; therefore its weight $r = b'\,u\,.\,100\,(\mathrm{M} - \mathrm{G}) = \dfrac{b\,u}{1 + 100\,\mathrm{M}}\,100\,(\mathrm{M} - \mathrm{G})$ $= \dfrac{p\,.\,100\,(\mathrm{M}-\mathrm{G})}{1 + 100\,\mathrm{M}}$. Therefore the true expansion of glass from 0° to 100°

$$100\,\mathrm{G} = 100\,\mathrm{M} - \frac{r}{p}\,(1 + 100\,\mathrm{M}).$$

The value of the true expansion of mercury is here assumed to be known. This may be done with confidence, inasmuch as it has been determined, quite independently of the expansion of glass, by the masterly experiments of Dulong and Petit. They found 100 M = 0·0180180. Therefore

$$100\,\mathrm{G} = 0{\cdot}018018 - 1{\cdot}018018\,\frac{r}{p}.$$

The following table exhibits the values of 100 M — 100 G for the glass employed, which was potash glass, from the manufactory at Reymyra. The first fifteen results were obtained from globes used in experiments upon the melting-points of easily fusible metals; the remainder were obtained from the globes used in determining the expansion of air. They show that the same kind of glass, though made at different times, and therefore in different meltings, possess the same expansibility.

·015732	·015720	·015732	·015713
·015744	·015761	·015706	·015697
·015754	·015730	·015731	·015751
·015744	·015711	·015741	·015744
·015723	·015737	·015753	·015726
·015735	·015720	·015762	·015736

The mean of the twenty-four results gives the difference between the true expansion of mercury and the expansion in volume of potash glass, 100 M − 100 G = 0·015733. Hence the true expansion in volume of the potash glass of Reymyra from 0° to 100°,

$$100\ G = 0·002285.$$

In the following table of the results of nine observations, p and $p - q$ are expressed in grammes, and h, k, l in centimetres.

p.	$p - q$.	h.	k.	l.	T.	100 A.
166·6891	133·1409	76·528	74·277	3·93	100·20	0·3643
173·4432	131·7215	76·362	77·584	3·81	100·13	0·3654
183·4963	143·2124	75·702	75·965	4·69	99·89	0·3644
154·2360	120·6356	77·230	75·910	3·50	100·45	0·3650
174·6862	134·9876	77·985	77·748	3·81	100·73	0·3653
187·4650	144·9009	76·444	76·474	3·81	100·16	0·3636
198·8099	172·7273	76·442	76·271	11·70	100·16	0·3651
184·4872	146·6123	75·811	75·342	5·25	99·93	0·3643
191·1037	178·9558	75·779	76·105	16·65	99·92	0·3645

The value of 100 A. in the sixth line is too small, in consequence of the loss of a globule of mercury in one part of the experiment. The mean of the preceding values of 100 A. is 0·3646.

Two other experiments were made with cylinders of glass. It was found impossible to boil the mercury contained in them, on account of the smallness of the bore of the tubes which formed their necks. The results are therefore considered less accurate.

p.	$p - q$.	h.	k.	l.	T.	100 A.
1158·902	946·516	76·773	76·789	7·80	100·28	0·3646
1196·992	991·695	76·313	75·470	7·92	100·12	0·3662
		Mean				0·3654

Two other observations were made without previously drying the air with chloride of calcium, in which, however, an examination with a microscope showed that there were no visible drops of water in the globe. These experiments were made merely

for the purpose of seeing how great an error might be introduced by neglecting to dry the air completely. The results are,

p.	*p* − *q.*	*h.*	*k.*	*l.*	T.	100 A.
166·4746	128·0336	75·166	75·049	4·21	99·69	0·3840
139·2725	106·1248	75·964	75·201	4·325	99·99	0·3902

The experiment was repeated with the ball used in the last of the above observations, the air having first been perfectly dried. It gave the following results:—

p.	*p* − *q.*	*h.*	*k.*	*l.*	T.	100 A.
139·2725	107·8192	76·440	76·185	3·725	100·16	0·3652

From the whole of these observations, I can come to no other conclusion, than that the expansion of dry air, and without doubt of all other dry gases, from 0° to 100°, is not 0·375, but only from 0·364 to 0·365 of the volume of the gas at 0°.

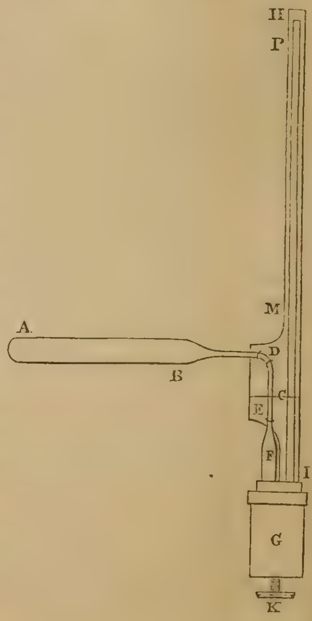
Article XV.

Second Series of Experiments on the Expansion of Dry Air between 0° *and* 100°. *By the late Professor* F. Rudberg.

[From Poggendorff's *Annalen*, B. 44. S. 119.]

SINCE the publication of my experiments on the expansion of air (Poggendorff's *Annalen*, B. 41. S. 271.), I have had an apparatus constructed, by the aid of which one such experiment may be made in the short space of an hour and a half, or two hours. The mean of the results which it has given agree perfectly with those I had previously obtained. I here communicate a short description of the apparatus, and the values of the expansion which it has afforded.

The construction of the apparatus enables us to determine the pressures of a given mass of dry air at 0° and at 100°, the spaces occupied by the air in the two cases differing only by the expansion of the receiver.



The dry air is contained in the cylinder A B, which communicates through the slender tube D E with the wide tube F, which, together with a second tube H I, about 50 centimetres long, and open at both ends, is cemented into the lid of the box G. The box contains a leathern bag for holding mercury, the capacity of which, as in a barometer, can be altered by means of the screw K, so that the mercury may be elevated or depressed in the tubes. A fine line is traced with a diamond point on the slender tube D E at C, up to which the mercury is screwed, as

well when the air in the receiver A B is cooled down to 0°, as when it is heated up to the boiling point of water. In order to measure with accuracy the altitude of the mercury in the tube, a brass scale H I, divided into millimetres, is attached to the tubes. The line which marks the commencement of the divisions at C is so long, that it passes behind both tubes, and thus the altitude of the extremity of the column of mercury in the tube H I, above the mark on D E at C, is easily determined.

The air in the receiver A B was dried before the tubes were cemented into the box, in the following manner. The lower end of the tube F was drawn out to a capillary point, and connected with a very wide tube filled with chloride of calcium, which communicated with an air-pump. After the air had been fifty times exhausted and re-admitted, the capillary point was sealed and the tube cemented into the box G, which had been previously filled with dried mercury, and lastly, the sealed end broken off under the surface of the mercury.

The capillary depression of mercury at C was determined by experiment before the narrow tube D E was joined to the receiver, and found equal to 1·85 centimetres.

The tube F was taken of large diameter, in order to receive the air as it expanded on being heated from 0° to 100°, and so obviate the necessity of continually screwing up the mercury.

The calculation and the method of observing are both equally simple. When the air in the receiver A B is cooled down to 0°, and the mercury is screwed up to C in the tube D E, let the mercury stand at M in the tube H I. At the same instant let h be the altitude of the mercury in the barometer. Let the altitude of the mercury in H I, above the mercury in D E, or C M $= k$, and let l be the capillary depression of the mercury in D E; then the pressure of the air in the receiver A B will be $h + k - l$. When afterwards the air is heated up to the boiling point of water, and the mercury is screwed up to C in the tube D E, let the mercury stand at P in the tube H I. At the same instant let h' be the height of the mercury in the barometer, and the difference of altitude of the mercury C P $= k'$; then the pressure of the air in the receiver will be $h' + k' - l$. Let T be the temperature of steam corresponding to the barometric height h', 100 A. the expansion of air from 0° to 100°, and 100 G. the expansion of glass in volume from 0° to 100°;

then

$$1 + \mathrm{AT} = \frac{h' + k' - l}{h + k - l} (1 + \mathrm{G\,T}).$$

In the above expression the altitudes h', k', h, k need not be connected for temperature, because the experiment is completed in the short space of an hour and a half, during which the temperature of a room will undergo no sensible variation. The only reduction to 0° requisite, is that of the barometric height h', in order to deduce from it the temperature T.

The experiments which I have made up to the present time, with the above-described apparatus, under very different barometric pressures (from $752^{\mathrm{mm}}\cdot92$ at + 17°·4 to $783^{\mathrm{mm}}\cdot72$ at + 18°), gave for 100 A. the following values :—

0·3640	0·3640	0·3653
0·3648	0·3656	0·3640
0·3641	0·3643	0·3664
0·3648	0·3648	0·3645.

The mean value of 100 A. is 0·36457. Since this mean value is the same as that given by my former experiments made in a manner entirely different, I venture to consider it as fully established,—that the true expansion of dry air between 0° and 100° centigrade, is only from 0·364 to 0·365 of its volume at 0°.

Article XVI.

On Barometrical Measurement of Heights. *By* F. W. Bessel.

[From the *Astronomische Nachrichten*, Nos. 356, 357.]

1.

The atmosphere of the earth is known to be composed of the nitrogen, oxygen, and carbonic acid gases, and of aqueous vapour. These constituents are supposed to exercise no chemical action on each other; and arbitrary quantities of them, mixed together under circumstances of equal temperature and pressure, occupy spaces equivalent to the sum of the spaces that they would severally occupy. Were we to assume that the constituents of the atmosphere are mixed in the same proportion at all times and at all altitudes, we might dispense with the knowledge of what that proportion is, in treating of the conditions of their equilibrium; but if we desire to preserve the freedom of founding our investigations on other suppositions also, we must not pass by in silence the mode in which the constituents are combined.

The proportion of the three gases may not always be exactly the same at a given point of the earth's surface; but the alterations which take place are so small, that they are only discoverable by chemical experiments frequently repeated; we cannot, therefore, regard the proportion as determinable by observation for each particular case, and we must assume a certain proportion.

According to Berzelius, the spaces occupied by the three gases, in the order in which they are named above, are to each other as

$$77{\cdot}96\,; \qquad 21{\cdot}15\,; \qquad 0{\cdot}07\,;$$

or, one volume of dry atmospheric air at the surface of the earth contains

$$v \;= 0{\cdot}78605 \text{ nitrogen gas}$$
$$v_{\prime} = 0{\cdot}21325 \text{ oxygen gas}$$
$$v_{\prime\prime} = 0{\cdot}00070 \text{ carbonic acid gas.}$$

The same great chemist has given the densities of these three gases, under the pressure which gives to the mixture the density D, viz.:

Nitrogen gas $= 0{\cdot}9691\ D = d\ D$
Oxygen gas $= 1{\cdot}1026\ D = d_{,}\ D$
Carbonic acid gas $= 1{\cdot}5260\ D = d_{,,}\ D$.

These six numbers require to be slightly altered, in order that they may correspond to the relation

$$1 = v\,d + v_{,}\,d_{,} + v_{,,}\,d_{,,}. \quad . \quad . \quad . \quad . \quad (1.)$$

Designating by M, m, $m_{,}$, $m_{,,}$, the masses, and by D, δ, $\delta_{,}$, $\delta_{,,}$, the densities of the mixture and of its constituent parts, we have, on the supposition of equal distribution in the space,

$$D : M = \delta : m = \delta_{,} : m_{,} = \delta_{,,} : m_{,,}; \quad . \quad . \quad . \quad (2.)$$

further, if P, p, $p_{,}$, $p_{,,}$ denote the pressures which the mixture and its constituent parts, the latter taken separately, exert on the unit of surface of the enclosing space, we have by Mariotte's law,

$$P : d\ D = p : \delta$$
$$P : d_{,}\,D = p_{,} : \delta_{,}$$
$$P : d_{,,}\,D = p_{,,} : \delta_{,,},$$

and also

$$P : 1 = p : v = p_{,} : v_{,} = p_{,,} : v_{,,}$$

thus

$$\delta = v\,d\,D; \qquad \delta_{,} = v_{,}\,d_{,}\,D; \qquad \delta_{,,} = v_{,,}\,d_{,,}\,D.$$

Introducing these values of δ, $\delta_{,}$, $\delta_{,,}$ into the above proportion (2.), we obtain

$$m = v\,d\,M, \quad m_{,} = v_{,}\,d_{,}\,M, \quad m_{,,} = v_{,,}\,d_{,,}\,M,$$

and as $M = m + m_{,} + m_{,,}$, we have also the relation (1). To satisfy this relation I have slightly altered d and $d_{,}$, making the first 0·9711, and the second 1·1048.

Biot and Arago determined the density of atmospheric air (*i. e.* the mixture of the three gases) at the surface of the earth, at the temperature of melting ice, and under a pressure of a column of mercury of the same temperature in the 45th parallel of latitude of 336·905 Parisian lines, to be 10466·8 times less than that of mercury. Under the aforesaid circumstances, therefore, $D = \frac{1}{10466{\cdot}8}$.

As the temperature increases, the specific elasticity of the air, or the space which a given quantity of air occupies, increases also, the pressure remaining equal. Gay Lussac arrived at the remarkable result that the specific elastic force of all gases and

vapours alters equally with equal changes of temperature, and that the alteration is proportional to degrees of the mercurial thermometer. If the elasticity at the temperature of melting ice be 1, and its alteration for a change of temperature corresponding to one degree of the thermometric scale $= k$, its value for a given amount of the thermometer is

$$E = 1 + k t.$$

For the temperature of boiling water Gay Lussac found E $= 1{\cdot}375$.

Besides the three gases the atmosphere contains aqueous vapour, which is present in variable quantities, determinable only by experiment in each particular case. I propose to return hereafter to this part of the subject; but I will first consider of atmospheric air unmixed with aqueous vapour.

2.

Barometric measurements of height rest on a comparison of the observed pressure of the atmosphere at different heights, with the expression denoting the conditions of its equilibrium. Although this expression has been developed in the *Mécanique Céleste*, and in several subsequent works, I shall not omit its development here; as it will enable me to introduce a small alteration, as well as to connect what I have further to say. Mariotte's law requires that to produce equilibrium the density (δ) of the air should be in the direct ratio of the pressure (p) which it experiences, and which it consequently exerts in return, and in the inverse ratio of its elastic force; or that $\frac{\delta . E}{p}$ be constant. The air is here supposed to be constituted alike at all altitudes. If we take for the measure of p the pressure exerted on an unit of surface, by a column of mercury of 336·905 Parisian lines, at the temperature of melting ice, at the surface of the earth in the latitude of 45°,—for the measure of δ the density of mercury at the temperature of melting ice,—and for the measure of E the specific elastic force of air at the same temperature,—and if we make $\delta = D$ for $p = 1$, and $E = 1$, we have

$$\delta . E = p D \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3.)$$

The pressure of the air at an elevation x above the surface of the earth, or at a distance $a + x$ from its centre, is the sum of the pressures of all the strata above x. A stratum between the

elevations x and $x + dx$ has for every unit of its surface the mass $\delta \,.\, dx$; therefore it exerts on this unit the pressure

$$(g)\ \delta \left(\frac{a}{a+x}\right)^2 dx,$$

in which (g) is gravity at that part of the earth's surface which is perpendicularly beneath the point to which x and δ belong, expressed in terms of gravity in the latitude of 45°. But in order that the diminution of pressure, caused by taking away this stratum from those above x, may be obtained in terms of the measure applied to p, the above expression must be divided by that measure, which then gives

$$dp = -\frac{(g)\ \delta}{336{\cdot}905} \left(\frac{a}{a+x}\right)^2 dx;$$

or, if we prefer the use of the toise to that of the Paris line,

$$dp = -\frac{(g)\ 864 \,.\, \delta}{336{\cdot}905} \left(\frac{a}{a+x}\right)^2 dx \quad . \quad . \quad (4.)$$

If we eliminate δ by combining the two equations, we obtain

$$\frac{dp}{p} = -\frac{(g)\ 864 \,.\, \mathrm{D}}{336{\cdot}905} \left(\frac{a}{a+x}\right)^2 \frac{dx}{\mathrm{E}}.$$

By the integral of this equation the values of p at two different elevations above the surface of the earth, $x = h$, and $x = h'$, become comparable with each other; or, if we denote them by P and P$'$, and employ Briggs's logarithms, of which the modulus is μ,

$$\log \frac{\mathrm{P}'}{\mathrm{P}} = -\frac{(g)\ 864 \,.\, \mathrm{D} \,.\, \mu}{336{\cdot}905} \int_h^{h'} \left(\frac{a}{a+x}\right)^2 \frac{dx}{\mathrm{E}};$$

or if we write

$$\frac{336{\cdot}905}{864 \,.\, \mathrm{D} \,.\, \mu} = 9397{\cdot}74 = l = (g)\ l'$$

then

$$\log \frac{\mathrm{P}}{\mathrm{P}} = -\frac{1}{l'} \int_h^{h'} \left(\frac{a}{a+x}\right)^2 \frac{dx}{\mathrm{E}} \quad . \quad . \quad . \quad (5.)$$

The integration, which still remains to be performed, requires that we know the relation between x and E, or the law according to which the observed heights of the thermometer τ and τ', corresponding to the temperature of the air at the two heights, pass into each other. We do not know this law in every case, and we have, therefore, no ground for assuming the change of temperature to be otherwise than proportioned to the change of ele-

vation. In order to correspond approximately to this view, and at the same time to give the integral the most simple form possible, Laplace assumes

$$(1 + k\,t)^2 + \frac{i\,a\,x}{a + x}$$

to be constant for all corresponding values of t and x, and determines the constant i, so that it may satisfy the two observed temperatures τ and τ'. Hence

$$(1 + k\,t)^2 + i\,\mathrm{X} = (1 + k\,\tau)^2 + i\,\mathrm{H} = (1 + k\,\tau')^2 + i\,\mathrm{H}'$$

where I have written X, H and H′ for $\frac{a\,x}{a + x}$, $\frac{a\,h}{a + h}$, $\frac{a\,h'}{a + h'}$.

We obtain thereby

$$i = \frac{2\,k\,(\tau - \tau')\left(1 + k\,\frac{\tau + \tau'}{2}\right)}{\mathrm{H}' - \mathrm{H}},$$

and

$$d\,\mathrm{X} = \left(\frac{a}{a + x}\right)^2 d\,x = -\,\frac{2\,k}{i}\,(1 + k\,t)\,d\,t;$$

and further,

$$\left(\frac{a}{a + x}\right)^2 \frac{d\,x}{\mathrm{E}} = -\,\frac{2\,k}{i}\,d\,t,$$

whence the integral taken from h to h' is

$$\frac{2\,k}{i}\,(\tau - \tau') = \frac{\mathrm{H}' - \mathrm{H}}{1 + k\,\frac{(\tau + \tau')}{2}}.$$

We have thus, in accordance with Laplace's assumption of the law of the change of temperature, transformed the formula (5.) into

$$\log\frac{\mathrm{P}'}{\mathrm{P}} = \frac{1}{l'}\cdot\frac{\mathrm{H}' - \mathrm{H}}{1 + k\,\frac{\tau + \tau'}{2}} \quad . \quad . \quad . \quad . \quad (6.)$$

3.

I have hitherto considered the air as *dry*, and have still to take into account the aqueous vapour which it always contains. If, in a circumscribed space, the mixture of the dry constituents of the atmosphere exert on the circumscribing surface the pressure p, the aqueous vapour the pressure $p_{,}$, and if the specific gravities of the two be respectively denoted by D and $d_{,}$D, and of the moist air which results from their mixture by D′, then according to equation (1.),

$$1 = v\frac{D}{D'} + v_{\prime}\frac{d_{\prime} D}{D'}$$

and

$$v = \frac{p}{p + p_{\prime}}, v_{\prime} = \frac{p_{\prime}}{p + p_{\prime}},$$

thus

$$D' = D\frac{p + p_{\prime} d_{\prime}}{p + p_{\prime}};$$

or if, to avoid introducing a new sign, we denote the whole pressure ($= p + p_{\prime}$) by p

$$D' = D\left\{1 - \frac{p_{\prime}}{p}(1 - d_{\prime})\right\} \ldots\ldots\ (7.)$$

For moist air, therefore, the equation (3.) is changed into

$$\delta . E = \{p - p_{\prime}(1 - d_{\prime})\} D,$$

and its combination with (4.) gives

$$0 = dp + \frac{1}{\mu l'} p \frac{dX}{E} - \frac{(1 - d_{\prime})}{\mu l'} p_{\prime} \frac{dX}{E}. \ \ldots\ (8.)$$

To integrate this equation, we must know the dependence which $p_{\prime}$ has on the other variable magnitudes. If in a particular case we have no observation determining the amount of aqueous vapour contained in the air, we must found our calculation on the supposition either of a mean state of the atmosphere, or of one which may appear more suitable to the actual circumstances. I will first examine the case in which we may suppose that at every point of the atmosphere there exists a determinate portion of the maximum quantity of vapour which it can receive in accordance with its temperature. If this maximum of vapour exert the pressure $(p_{\prime})$, I then assume

$$p_{\prime} = \alpha (p_{\prime}),$$

where by α I understand a constant factor not greater than unity, the value of which is to be determined hereafter.

The expression for $(p_{\prime})$, at the given t of the centesimal scale, deduced by Laplace (*Méc. Cél.*, iv. p. 273.) from the experiments of Dalton, in the unit of pressure chosen in the foregoing article,

$$= 10^{(t - 100)\, 0{\cdot}0154547 - (t - 100)^2\, 0{\cdot}0000625826}.$$

For which we may also write

$$(p_{\prime}) = 0{\cdot}0067407 \,.\, 10^{t \cdot 0{\cdot}0279712 - t^2\, 0{\cdot}0000625826} \ldots\ (9.)$$

We have thus, conformably to the supposition,

$$p_{\prime} = \alpha\beta\, 10^{at - ct^2},$$

where
$$\beta = 0{\cdot}0067407$$
$$a = 0{\cdot}0279712$$
$$c = 0{\cdot}0000625826.$$

If we now multiply the differential equation (8.) by

$$10^{\frac{1}{l'}\int\frac{d\mathrm{X}}{\mathrm{E}}}$$

we can integrate the product, namely,

$$\mathrm{C} = p \,.\, 10^{\frac{1}{l'}\int\frac{d\mathrm{X}}{\mathrm{E}}} - \frac{(1-d_{\prime})}{\mu\, l'}\int p_{\prime}\, 10^{\frac{1}{l'}\int\frac{d\mathrm{X}}{\mathrm{E}}}\,\frac{d\mathrm{X}}{\mathrm{E}}.$$

Laplace's assumption of the law of the change of temperature between two elevations, at each of which the temperature is given by observation (Art. 2.), is

$$\frac{d\mathrm{X}}{\mathrm{E}} = -\frac{2\,k}{i}\,d\,t.$$

If we substitute this, and also the expression above given of $(p_{\prime})$, we have

$$\mathrm{C} = p \,.\, 10^{-\frac{2\,k}{i\,l'}\,t} + \frac{2\,\alpha\,\beta\,(1-d_{\prime})\,k}{\mu\, l'.\, i}\int 10^{\left(a-\frac{2\,k}{l'.i}\right)t - c\,t^2}\,d\,t.$$

By this equation we obtain the relation between the pressures of the atmosphere P and P′ at the heights h and h', namely,

$$\mathrm{P} \,.\, 10^{-\frac{2\,k}{l'\,i}\,\tau} - \mathrm{P}' .\, 10^{-\frac{2\,k}{l'\,i}\,\tau'}$$
$$= \frac{2\,\alpha\,\beta\,(1-d_{\prime})\,k}{\mu\, l'\, i}\int_{\tau}^{\tau'} 10^{\left(a-\frac{2\,k}{l'\,i}\right)t - c\,t^2}\,d\,t.$$

If we write T for $\frac{1}{2}\,(\tau+\tau')$, and $\mathrm{T}+z$ for t, then the integral still to be sought is changed into

$$-\,10^{\frac{2\,k}{l'\,i}\mathrm{T}} .\, 10^{a\,\mathrm{T} - c\,\mathrm{T}^2}\int_{-\frac{1}{2}(\tau-\tau')}^{\frac{1}{2}(\tau-\tau')} 10^{\left(a\cdot\frac{2\,k}{l'\,i} - 2\,c\,\mathrm{T}\right)z - c\,.\,z^2}\,d\,z.$$

If, for brevity, we write

$$u = \frac{2\,\alpha\,\beta\,(1-d_{\prime})\,k}{l'.\,i}\,10^{a\,\mathrm{T} - c\,\mathrm{T}^2}\int_{-\frac{1}{2}(\tau-\tau')}^{\frac{1}{2}(\tau-\tau')} 10^{\left(a-\frac{2\,k}{l'.i} - 2\,c\,\mathrm{T}\right)z - c\,z^2}\,d\,z,$$

We have thus,

$$\mathrm{P} \,.\, 10^{-\frac{2\,k}{l'\,i}\,\tau} - \mathrm{P}' .\, 10^{-\frac{2\,k}{l'\,i}\,\tau'} = -\,\frac{u}{\mu}\,10^{-\frac{2\,k}{l'\,i}\,\mathrm{T}},$$

or

$$P \,.\, 10^{-\frac{2k}{l'i}(\tau - \tau')} + \frac{u}{\mu} 10^{-\frac{2k}{l'i}\left(\frac{\tau - \tau'}{2}\right)} = P';$$

whence it follows that

$$10^{-\frac{2k}{l'i}(\tau - \tau')} = \frac{P' \sqrt{[4\mu^2 PP' + u^2]} - u}{P \sqrt{[4\mu^2 PP' + u^2]} + u};$$

and if we take the Briggs's logarithms of both numbers of the equation, and develope fully to u^2 inclusive,

$$\frac{2k}{l'i}(\tau - \tau') = \log \frac{P}{P} + \frac{u}{\sqrt{(PP')}};$$

but according to the relation between the temperatures and the elevations in Art. 2,

$$\frac{2k}{i}(\tau - \tau') = \frac{H' - H}{1 + kT};$$

whereby we obtain

$$\log \frac{P}{P'} = \frac{1}{l'} \frac{H' - H}{1 + kT} - \frac{u}{\sqrt{(PP')}}. \quad . \quad . \quad . \quad (10.)$$

The integral occurring in the expression for u is found by developing the exponential quantity into a series

$$= (\tau - \tau') \left\{1 + \frac{(\tau - \tau')^2}{24\mu^2}\left[\left(a - \frac{2k}{l'i} - 2cT\right)^2 - 2c\mu\right] + \&c\ldots\right\}.$$

In order to estimate in some measure the amount of the second member of this series, we may assume that the centesimal thermometer falls a degree for every 85 toises of elevation. Then is this member for $H' - H = n \,.\, 1000$ toises, and for $T = 0$, $= n^2 \,.\, 0{\cdot}0093$ for small differences of elevation; it is therefore an inconsiderable part of the first member; and even for the greatest accessible elevations it does not amount to a tenth part of it. The supposition as to the distribution of aqueous vapour in the atmosphere, on which the present calculation rests, has far more uncertainty; on which account I think there can be but little interest in adhering strictly to it by means of a complicated calculation. I therefore simplify it by assuming

$$u = \frac{2\alpha\beta(1 - d_{\prime})k(\tau - \tau')}{l'i} 10^{aT - cT^2}.$$

According to Berzelius $d_{\prime} = 0{\cdot}62$, and it has been shown above that

$$\beta = 0{\cdot}0067407,$$

$$\frac{2\,k\,(\tau - \tau')}{i} = \frac{\mathrm{H}' - \mathrm{H}}{1 + k\,\mathrm{T}}.$$

Hence

$$u = \alpha\,\frac{\mathrm{H}' - \mathrm{H}}{l'}\,.\,\frac{10^{a\,\mathrm{T} - c\,\mathrm{T}^2}}{1 + k\,\mathrm{T}}\,0{\cdot}002561\,;$$

and we obtain, by the substitution of this expression in equation (10.),

$$\log\frac{\mathrm{P}}{\mathrm{P}'} = \frac{\mathrm{H}' - \mathrm{H}}{l'\,(1 + k\,\mathrm{T})}\left\{1 - \alpha\,.\,\frac{0{\cdot}002561}{\sqrt{(\mathrm{P}\,\mathrm{P}')}}\,10^{a\,\mathrm{T} - c\,\mathrm{T}^2}\right\}\,..\,(11.)$$

If we wish to found the calculation of the difference in height of two points, where the pressures and temperatures of the air have been observed, upon the supposition of a mean state between dryness and saturation, we must make $\alpha = \frac{1}{2}$. But if we have not an immediate determination of the quantity of aqueous vapour on such occasions, we may obtain in particular cases, by taking other circumstances into account, greater exactness than by making $\alpha = \frac{1}{2}$. If, for example, rain falls throughout the whole space between the two elevations, then $\alpha = 1$. If the two points are far distant from the ocean, and in a country known to be particularly dry*, it will be more suitable to take α less than $\frac{1}{2}$. In order to give a direct view of the influence of aqueous vapour on barometric measurement, I will develope it further. The increase, which is occasioned in a difference of elevation computed on the supposition of dry air, by the introduction of the consideration of the aqueous vapour, according to equation (11.), is

$$= \frac{\alpha\,w}{1 - \alpha\,w}\,(\mathrm{H}' - \mathrm{H})$$

where w is written for

$$\frac{0{\cdot}002561}{\sqrt{(\mathrm{P}\,\mathrm{P}')}}\,10^{a\,\mathrm{T} - c\,\mathrm{T}^2}.$$

If we neglect the square of this quantity, and make

$$\mathrm{P}' = \mathrm{P}\,10 - \frac{(\mathrm{H}' - \mathrm{H}}{l'\,(1 + k\,\mathrm{T})},$$

which can only occasion an error of the order w^2,

$$w = \frac{0{\cdot}002561}{\mathrm{P}}\,10^{\frac{\mathrm{H}' - \mathrm{H}}{2\,l'\,(1 + k\,\mathrm{T})}}\,.\,10^{a\,\mathrm{T} - c\,\mathrm{T}^2}$$

* Such is the case in a great part of northern Asia, as we learn from Adolphe Erman's Reise, vol. ii. p. 67, where we have not only the fact, but the geographical relations of which it is the consequence.

and thence the influence of the aqueous vapour

$$= \alpha . \frac{0{\cdot}002561}{P} (H' - H) \, 10^{\frac{H'-H}{2 l'(1+kT)}} \, . \, 10^{a\,T - c\,T^2} \ldots \quad (12.)$$

If we assume the pressure in this formula at the height $h = 1$, or the height of the barometer there $= 336_L{\cdot}905$, and $k = 0{\cdot}00375$, we find the quantities to be multiplied into α for different values of $H' - H$ and T as they are given in the following table.

$$T = \tfrac{1}{2} (\tau + \tau').$$

H′ − H.	0°.	10°.	20°.
T	T	T	T
500	1·36	2·55	4·64
1000	2·90	5·41	9·83
1500	4·62	8·61	15·60
2000	6·55	12·18	22·02
2500	8·70	16·15	29·14
3000	11·10	20·15	37·02

From these numbers we may judge of the influence on the result which may be occasioned by an uncertainty in the value of α in any occurring case.

4.

Since the invention of Daniell's Hygrometer and of August's Psychrometer, we have the means of ascertaining at all times, with ease and sufficient exactness, the quantity of aqueous vapour contained in the atmosphere. The observation of the psychrometer at both elevations, in addition to those of the barometer and thermometer, is readily made, and dispenses with any arbitrary supposition in regard to the moisture, as that of the thermometer does in regard to the temperature of the air. I will, therefore, examine the rules of calculation which are applicable in cases where the psychrometer has been observed.

The psychrometer rests on the comparison of the heights $\tau_{\prime}$ and τ of two thermometers, one with a moistened bulb, and the other with a dry bulb. If the greatest pressure which aqueous vapour at the temperature t can exert be denoted by $\phi\, t$, and the height of the barometer in Parisian lines by b, the existing pressure of the vapour

$$= \phi\, \tau_{\prime} - \frac{0{\cdot}558\, (\tau - \tau_{\prime})\, b}{336{\cdot}905\, (m - \tau_{\prime})},$$

where, if the value of τ be positive, $m = 640$; and if negative

(in which case the moistened bulb is coated with ice), $m = 715$. This formula is given by August, the inventor of the psychrometer, and rests on the comparison of experiments with certain physical considerations*.

The expression for $\phi\, t$ for different values of t is deduced by August from observations on the pressure exerted by aqueous vapour at different temperatures, employing a mean result, which Kämptz has derived from the observations of Dalton, Ure, Schmidt, and Artzberger. But these, with the exception of the two first-named series, are so little accordant with each other, that it may be doubtful whether all the four should be combined. I prefer to adhere to the expression already given, which Laplace derived from Dalton's observations, to which those of Ure approximate. It is my opinion generally that formulæ which are well known and extensively applied ought not to be altered until the necessity for the alteration becomes decided, which is by no means the case in the present instance. The researches since made by Arago on the same subject were confined to the elastic force of aqueous vapour at very high temperatures, and we cannot be sure that a formula of interpolation, which represents those satisfactorily, is applicable in much lower degrees of temperature.

By applying Laplace's expression, we obtain the pressure exerted by the aqueous vapour contained in the atmosphere according to the formula which has been already given.

$$p_{\prime} = 0{\cdot}0064707 \,.\, 10^{a\,\tau_{\prime} - c\,\tau_{\prime}^2} - 0{\cdot}0016562\, \frac{(\tau - \tau_{\prime})\, b}{m - \tau_{\prime}};$$

and if we divide it by

$$(p_{\prime}) = 0{\cdot}0064707 \,.\, 10^{a\,\tau - c\,\tau^2},$$

or by the pressure which the vapour would exert if the air were saturated with it, we obtain the proportion denoted above by α, thus:

$$(13.) \quad \ldots\ldots \alpha = \frac{10^{a\,\tau_{\prime} - c\,\tau_{\prime}^2}}{10^{a\,\tau - c\,\tau^2}} - 0{\cdot}2457\, \frac{b}{m - \tau_{\prime}} \cdot \frac{\tau - \tau_{\prime}}{10^{a\,\tau - c\,\tau^2}}.$$

To facilitate the calculation of α I subjoin a table, the first section of which is for all values of t from $-20°$ to $+30°$, containing

$$\log 10^{a\,t - c\,t^2} = f\,t,$$

* Poggendorff Ann. der Physik, vol. lxxxi. p. 69, and vol. xc. (xiv. of the new series), p. 137.

and the second section

$$\log \frac{0{\cdot}2457}{m-t}\, 10^{-at+ct^2} = \psi\, t.$$

We obtain thereby

$$\log A = f\tau_{\prime} - f\tau$$

$$\log B = \log A + \psi\,\tau_{\prime} + \log\,(\tau - \tau_{\prime}) + \log b,$$

and α, which is sought, $=$ A $-$ B.

t.	ft.		ψt.		t.	ft.		ψt.	
$-20°$	9·4155	304	7·1086	298	$+5°$	0·1383	273	6·4493	266
-19	9·4459	303	7·0788	297	6	0·1656	271	6·4227	264
-18	9·4762	302	7·0491	296	7	0·1927	271	6·3963	264
-17	9·5064	300	7·0195	295	8	0·2198	269	6·3699	262
-16	9·5364	299	6·9900	293	9	0·2467	268	6·3437	261
-15	9·5663	298	6·9607	292	10	0·2735	266	6·3176	260
-14	9·5961	297	6·9315	290	11	0·3001	265	6·2916	258
-13	9·6258	295	6·8925	290	12	0·3266	264	6·2658	257
-12	9·6553	294	6·8735	288	13	0·3530	263	6·2401	256
-11	9·6847	293	6·8447	287	14	0·3793	262	6·2145	255
-10	9·7140	292	6·8160	285	15	0·4055	260	6·1890	253
-9	9·7432	290	6·7875	285	16	0·4315	259	6·1637	252
-8	9·7722	289	6·7590	283	17	0·4574	258	6·1385	251
-7	9·8011	288	6·7307	282	18	0·4832	257	6·1134	249
-6	9·8299	287	6·7025	280	19	0·5089	255	6·0885	249
-5	9·8586	286	6·6745	279	20	0·5344	254	6·0636	247
-4	9·8871	284	6·6466	278	21	0·5598	253	6·0389	246
-3	9·9155	283	6·6188	277	22	0·5851	251	6·0143	244
-2	9·9438	282	6·5911	276	23	0·6102	251	5·9899	243
-1	9·9720	280	6·5635	274	24	0·6353	249	5·9656	242
0	0·0000	279	6·5361 / 6·5842	— 272	25	0·6602	247	5·9414	241
$+1$	0·0279	278	6·5570	271	26	0·6849	247	5·9173	239
2	0·0557	277	6·5299	270	27	0·7096	245	5·8934	239
3	0·0884	275	6·5029	268	28	0·7341	244	5·8695	237
4	0·1109	274	6·4761	268	29	0·7585	243	5·8458	235
5	0·1383		6·4493		30	0·7828		5·8223	

If we determine in this manner the values of α for two elevations, they will seldom be found of equal amount; as the law

of the transition of the one to the other is not known, we are compelled to decide arbitrarily, and it seems to me most suitable to take the mean of the two values of α to be applied in the calculation of the formula (11.).

5.

It appears to me needful to examine more closely the different suppositions by means of which I have obtained the formula (11.). The first assumption in all researches relating to the pressure and density of the atmosphere at undetermined heights, is that of its equilibrium. That this is not strictly correct, is not now said for the first time. Its incorrectness is shown both in the oscillations of the barometer around its mean height at each point of the earth, and in the difference of this height at different points strictly at the level of the sea. The knowledge of this difference was first obtained by an investigation by Adolphe Erman in 1831*, in which he showed, partly from his own observations made in his travels round the earth, and partly from the observations of others in Northern Asia and America, and on board the Russian corvette Krotkoi commanded by Captain Hagemeister, first, that in the zones of the trade winds, the barometer stands higher at the boundary most distant from the equator than at the boundary which is nearest to it; and secondly, that the mean height of the barometer is different in different meridians. The first result rests on observations collected in passing eight times through the zone of the trade winds; and has since been corroborated in Herschel's astronomically-memorable voyage to the Cape of Good Hope. The second result rests on a comparison of observations made in the Atlantic and Pacific Oceans; the differences amount to several lines, and leave no doubt that the mean height of the barometer at the level of the sea is different at different points of the earth's surface, and depends on the geographical latitude and longitude of the place.

The oscillations of the barometer, which may be regarded as *accidental*, must cause single barometrical determinations of a difference of elevation to deviate from the mean of several determinations; but the *mean* diversities, which depend on the longitude and latitude, if not known, must produce errors, which will not disappear in the mean even of many observations,

* Poggendorff, Ann. der Physik, vol. xcix. (xxiii. of the new series), p. 144.

except in the case when the two points, of which the difference of elevation is to be measured, are in the same perpendicular. It follows, from the knowledge we have obtained of these diversities, that barometrical determinations of the difference of elevation of two points, even if resting on observations repeated for years, remain the more doubtful the more distant the points are from each other. If we imagine surfaces surrounding the earth in which the mean pressure of the atmosphere is equal, then all we obtain by the barometer is the determination of differences of elevation relatively to these surfaces; but whether the surfaces at which the two points are situated differ more or less from parallelism with the surface of the earth, remains wholly unknown to us, whilst we are ignorant of the function of longitude and latitude which determines their relative position. This opens a new view in regard to observations on the pressure of the atmosphere; we have to examine for all points of the earth the height of the [atmospheric] surface at which a determinate [mean] pressure is found; but we cannot as yet determine more nearly, the amount of uncertainty arising from the assumption of this height being everywhere the same. Also the uncertainty, arising from the oscillations considered as accidental, cannot be given more nearly; and even if, for the purpose of learning them more correctly, we were to make long-continued observations at points at different elevations, the differences which might appear could still only be regarded as caused by the combination of these causes with other as unavoidably erroneous assumptions.

The constitution of dry air has been assumed such as it was supposed to be at the time that Biot and Arago obtained for its density the determination given in the first section. If the proportion of oxygen were to be altered by n hundredths, its density would be changed by $n \cdot 0{\cdot}001337$, and a difference of elevation computed under the assumption of $D = \frac{1}{10466{\cdot}8}$ would require to be altered in the ratio of $1 : 1 + n \cdot 0{\cdot}001337$. Humboldt and Gay Lussac, in nineteen days, between the 17th Nov. and 23rd Dec. 1804, found no sensible alteration in the proportion of oxygen, which seems to justify the assumption of a *constant* proportion in the two principal constituents of the atmosphere.

In the meatime, however, it is known that Dalton has con-

sidered it probable that each of the constituents of the air is compressed by its own superior strata alone, and not by the whole superincumbent mass; consequently, that at different heights each constituent possesses the density which it would have if it existed alone. Hence it must result, that the proportions of the mixture would vary with the altitude, and the relation of the atmospheric pressure at different heights would differ from the older assumption adopted in Sect. 2. Barometrical measurements of heights have been proposed as a means of deciding between the two assumptions. The attention, which the opinions of so eminent a physicist as Dalton deserve, requires that I should follow out his supposition also.

The formula (6.) then is no longer correct for the air generally, but only for each of its constituents; it applies to each of these according as the specific gravity of each is taken instead of that of the atmosphere itself. If we call the pressure exerted by the three constituents of the air, at the elevations h and h, $= p, p_{\prime}, p_{\prime\prime}$, and $p', p_{\prime}', p_{\prime\prime}'$, and their specific gravities D d, D $d_{\prime}$, D $d_{\prime\prime}$, and if for brevity we denote by U,

$$\frac{1}{l'} \cdot \frac{H' - H}{1 + k T};$$

then, according to formula (6.),

$$p' = p \;.\, 10^{-Ud}$$
$$p_{\prime}' = p_{\prime} \,.\, 10^{-Ud_{\prime}}$$
$$p_{\prime\prime}' = p_{\prime\prime} \,.\, 10^{-Ud_{\prime\prime}}$$

and as

$$P = p + p_{\prime} + p_{\prime\prime}$$
$$P' = p' + p_{\prime}' + p_{\prime\prime}'$$
$$p = v\,P;\; p_{\prime} = v_{\prime}\,P;\; p_{\prime\prime} = v_{\prime\prime}\,P;$$

therefore

$$P' = P\left\{v.\,10^{-Ud} + v_{\prime}.\,10^{-Ud_{\prime}} + v_{\prime\prime}.\,10^{-Ud_{\prime\prime}}\right\}$$

or

$$P' = P.\,10^{-U}\left\{v.\,10^{U(1-d)} + v_{\prime}.\,10^{U(1-d_{\prime})} + v_{\prime\prime}.\,10^{U(1-d_{\prime\prime})}\right\}$$

instead of which, we may write for brevity

$$P' = P\,.\,10^{-U}\,.\,\psi. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (14.)$$

The quantity ψ, at all accessible elevations, differs little from 1, as is shown by the following table, calculated according to the values given in the 1st Section, viz.

$$v = 0{\cdot}78605 \qquad d = 0{\cdot}9711$$
$$v_{\prime} = 0{\cdot}21325 \qquad d_{\prime} = 1{\cdot}1048$$
$$v_{\prime\prime} = 0{\cdot}00070 \qquad d_{\prime\prime} = 1{\cdot}5260$$

U.	ψ.	Log ψ.
0·0	1·0000000	0·0000000
0·1	1·0000840	0365
0·2	1·0003334	1148
0·3	1·0007444	3232
0·4	1·0013135	5701
0·5	1·0020375	8840

If the relation of P′ to P has been obtained by observation, and if the U proportionate to the difference of elevation be sought, this table shows that, according to Dalton's views, it will be found somewhat greater than according to the older supposition, and in a proportion given by the table, the numbers of which progress nearly as the square of the argument. If we are willing to be content with an approximation which scarcely differs from the truth in all cases of probable occurrence, we may develope (14.) further. We have

$$U = \log \frac{P}{P'} + \log \psi;$$

and if for log ψ we write the first member of its development, or

$$\frac{U^2}{2\mu}\left\{v(1-d)^2 + v_{\prime}(1-d_{\prime})^2 + v_{\prime\prime}(1-d_{\prime\prime})^2\right\} = U^2 . 0{\cdot}003675,$$

and for U, its expression,

$$\frac{H'-H}{l'(1+kT)} = \log.\frac{P}{P'} + \left\{\frac{H'-H}{l'(1+kT)}\right\}^2 0{\cdot}003675.$$

The alteration which the adoption of Dalton's view of the constitution of the atmosphere produces in the values of H′ − H calculated on the older supposition, is therefore

$$+\frac{(H'-H)^2}{l'(1+kT)}\,0{\cdot}003675,$$

and if $H' - H = n . 1000$ toises

$$= + \frac{n^2 . 0^{T}{\cdot}391}{1+kT} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (15.)$$

This difference is much too small for us to hope to obtain by barometrical measurements a decision for or against the reason-

ing on which it is founded: it is far exceeded by the constantly existing disturbances of the equilibrium of the atmosphere, as well as by the uncertainty of the law (applied in the 2nd Sect.) of the variation of temperature between two heights at which the thermometer has been observed. Even the *geometrical* measurement of the difference of elevation could scarcely be made with sufficient certainty to determine a quantity so small as that upon which a decision between the two assumptions would depend.

6.

The necessary following out of Dalton's supposition, in its relation to barometrical measurements of altitude, gives me an occasion of expressing my own view of this much-discussed subject. The supposition rests principally on the comportment of aqueous vapour when mixed with air, and when by itself.

A fluid brought into an empty space gives off vapour until the vapour has attained a density dependent on the temperature of the space. Dalton has determined this density, in the case of the vapour of water, for all degrees of temperature between freezing and boiling water; and has shown by indubitable experiments, that the vapour attains precisely the same density when the space is occupied by dry air, of any density whatsoever, as when it is originally a vacuum. Every attempt to increase the density, when the temperature remains the same, fails. If the space, when filled with the densest vapour consistent with the temperature, be contracted in the ratio of $1 : 1 - n$, a part of the vapour, proportioned to the whole as $n : 1$, is converted into fluid: precisely the same change takes place if a space filled with the densest vapour consistent with the temperature, and containing air of any density whatsoever, be contracted in the same proportion: in such case the air undergoes no corresponding change, but merely an increase of density in the ratio of $1 - n : 1$. This is the pure result of Dalton's experiments. They show a difference between vapour and air, assigning to vapour a maximum of density dependent on temperature, which does not exist in the case of air. They show further, that the forces at the surface of the fluid, which cause it to rise in vapour in a vacuum, are not counteracted by the pressure of the air in contact with it. In respect to the latter point, I may remark that Poisson derived from phænomena of another class, *i. e.* the capillary, that the density at the surfaces of fluids is in-

finitely small. Whether all gases have a maximum of density dependent on temperature (as is known to be the case with carbonic acid gas), so that they only differ from vapours by the amount of the maximum (or specifically), cannot at present be decided, and is not here touched on.

So long as vapours have a less density than the greatest they can attain in the respective temperatures, they are not physically different from gases; they follow Mariotte's law; and Gay Lussac has shown that they possess the same expansibility by temperature which is common to all gases. So long, therefore, as they have not attained the maximum of their density, they comport themselves, whether alone or mixed with gases, precisely as gases do. A *pressure* does not produce a change of state in them any more than in gases: that change first takes place, equally whether they are mixed or unmixed, whenever an attempt is made to cause their density to exceed its maximum. This can be done by lessening the space in which they are contained, in which case the gases, if present, remain unchanged in consequence of their unlimited compressibility. If, further, a space is filled with a gas which exerts a pressure p upon an unit of surface, the introduction of another gas, which if alone would exert the pressure $p_{\prime}$ on the same unit, produces no other physical consequence than that this unit now sustains the pressure $p + p_{\prime}$; but it would sustain precisely the same pressure, if, instead of the second gas, a vapour were introduced exerting when alone the pressure $p_{\prime}$. Lastly, different kinds of gases mix with each other, as well as with vapour, in any arbitrary proportions.

There is therefore throughout, no difference between the physical comportment of a mixture of two gases, and of a gas and vapour; consequently the circumstances of the second mixture can teach us nothing which we might not learn from those of the first. The comportment of the mixture of air and aqueous vapour, which Dalton's experiments have fully manifested, is not, therefore, more instructive than that of any mixture of two gases; and a theory which could not be constructed upon the latter, cannot find support in the former. It could not, therefore, have been deduced from the comportment of a mixture of vapour and air that the air does not press the vapour, unless for the presupposition that *pressure* changes vapour into fluid; for this presupposition, however, there is no justifying fact.

According to this view of our knowledge of vapour, no ground is afforded for the hypothesis that vapour is compressed only by vapour, and not by air; and we lose at the same time the analogy for the similar comportment of the mixture of different gases. Dalton has adduced, in further support of his supposition, a circumstance which is independent of the experiments on aqueous vapour, viz. that a specifically heavier gas mixes with a lighter one, even though the latter should be placed uppermost. It is true that Dalton's hypothesis explains this fact; but it cannot be maintained that the fact is inconceivable apart from the hypothesis. The ascent of fluids in tubes which are wetted by them might, for example, be explained by the assumption that gravity exerted its action but imperfectly within the tubes; but we know the true explanation is different. If I do not mistake, the small amount of the alteration which the constitution of the air undergoes in a space in which there are many persons, whose breathing must diminish the oxygen and increase the carbonic acid gas, has been adduced in support of Dalton's views, as the oxygen must *by preference* replace itself from the outward air, and the carbonic acid gas must pass to the same *in preference*, if the several constituents of the interior air are compressed only by those of the same nature without. The first experiments of the kind were made by Humboldt and Gay Lussac in one of the Parisian theatres*; and these gave a diminution of the oxygen of 0·007, with an imperfectly determined content of carbonic acid gas. Dalton subsequently repeated experiments of a similar kind† in spaces filled with numerous assemblages, and found the oxygen = 0·20325, whereas in free air he found it 0·2090; there was also more carbonic acid gas than in the free air, and in one case, in which it was determined, the amount was 0·01. These experiments do therefore show actual alterations in the constitution of the air; and it only remains to examine whether they are *less* than the alterations to be expected according to the older views. The first-mentioned experiments do not appear to have been made for the purpose of testing these; and all are deficient in the exact data requisite for founding a calculation; *i. e.* the cubic contents of the room, the air of which was examined,—the number of persons, and of the lights, and the strength of

* Gilbert's Ann. der Physik, vol. xx. p. 88.

† Phil. Trans. 1837, part II. p. 363.

the latter,—the communications with the external air,—and the temperature at different heights. Nor is the case examined sufficiently simple to be a fit subject for strict calculation. But to obtain an approximate view, I have proceeded from the rule adopted in Prussian towns, which prescribes that in buildings, which are to contain assemblages of people, not less than 100 cubic feet shall be allowed for each person. I have further diminished this space by one-third, and have taken Davy's experiments*, which show that each person diminishes in one minute the nitrogen by 4·9 cubic inches, and the oxygen by 19·5 cubic inches, and increases the carbonic acid gas by 15·4 cubic inches. If we assume that the diminution of 9 cubic inches is compensated by the necessary inpressing of the external air, on account of the continual augmentation of temperature which takes place, we find from these numbers that the proportion of the three gases of the atmosphere given in the first article, viz.

$$v = 0{\cdot}78605$$
$$v_{,} = 0{\cdot}21325$$
$$v_{,,} = 0{\cdot}00070$$

will be altered in the course of an hour to

$$v = 0{\cdot}78719$$
$$v_{,} = 0{\cdot}20405$$
$$v_{,,} = 0{\cdot}00875.$$

If we deduct from the mixed air the carbonic acid gas, the proportion of the two other gases is at first as 0·7866 : 0·2134, and at the end of an hour as 0·7941 : 0·2059. The calculated result is not so dissimilar from the experiment as to afford a conclusion that the supposition on which the calculation is founded is incorrect. It would, indeed, seem as if the comparison might rather be alleged against Dalton's view than in favour of it. I believe that if we desire decisive experiments on this point, they would most easily be obtained by observing the ingress, from pressure, of atmospheric air into a closed space not air-tight, and filled either with one of the constituents of the air, or with both mixed in a different proportion from that in which they exist in the atmosphere. In order to simplify the experiments, and to obtain most conveniently the bases of their calculation, the space ought not to be the interior of a building, but that of a bell glass.

* According to Gilbert's calculation, Ann. der Physik, vol. xix. p. 312.

If no special hypothesis be made as to the molecular constitution of gases and vapours, it is plain that a particle of gas must press an adjoining particle of similar or dissimilar constitution with equal force (*i. e.* with the same force with which it endeavours to expand). Without a special hypothesis Dalton's view contradicts the fundamental propositions of aerostatics. But such a view cannot be maintained unconditionally until proof is adduced that no supposition, such as is here referred to, is *mathematically* possible. On the other hand, the view which I have developed of the comportment of vapours, does not require to be justified by a special hypothesis. We may regard, as the immediate result of experiment, and as the distinguishing mark between vapours and gases, that the density of vapours cannot be increased beyond a certain degree dependent on temperature. But if we desire to enter likewise on the molecular constitution, it is easily conceivable that there may exist a distance between the ultimate particles of vapours, in which their attractive force is equal to the repulsive force arising from the temperature, so that every decrease of distance renders the attractive force predominant, and consequently unites the particles.

7.

If, notwithstanding what is here said, I have followed Dalton's view in Sect. 5, in respect to the dry constituents of the air, I can the less omit to examine the deductions from it in regard to the aqueous vapour. This examination must also be pursued, if we desire to learn whether the observed distribution of the aqueous vapour in the atmosphere can be made to tell for or against Dalton's hypothesis. I will, therefore, assume with Dalton the aqueous vapour in the atmosphere to be pressed only by its own higher strata, or to form an atmosphere by the equilibrium of its own parts alone. The change of the pressure of the atmosphere of vapour, corresponding to the increase $d\,x$ of the elevation x, is according to formula (4.),

$$d\,p_{\prime} = -\frac{(g)\,864\,.\,\delta}{336{\cdot}905}\left(\frac{a}{a+x}\right)^2 d\,x;$$

or, according to the notation subsequently introduced,

$$d\,p_{\prime} = \frac{\delta}{\mu\, l'\,\mathrm{D}}\,d\,\mathrm{X}.$$

Its density δ, until it reaches its maximum, follows Mariotte's

law, or corresponds to equation (3.), which, for the present case, is

$$\delta . \mathrm{E} = p_{\prime} \mathrm{D} \, d_{\prime};$$

to which must still be added the condition requisite for equilibrium, that the δ resulting from this equation shall at no elevation exceed the maximum of density corresponding to the temperature; or, according to the notation in Sect. IV., that

$$p_{\prime} \leqq \phi \, t.$$

If we eliminate δ, we obtain

$$\frac{d p_{\prime}}{p_{\prime}} = - \frac{d_{\prime}}{\mu \, l'} \, \frac{d \mathrm{X}}{1 + k t};$$

a similar differential has already been integrated in Sect. 2, assuming the variation of temperature between the two heights at which it was observed, to be that supposed by Laplace. With this assumption, it follows that

$$\frac{d \mathrm{X}}{1 + k t} = - \frac{2 k}{i} \, d t,$$

and thence the integral, reckoned from the elevation h, where τ is the height of the thermometer, and $\mathrm{P}_{\prime}$ the pressure of the aqueous vapour, is

$$\log \frac{\mathrm{P}_{\prime}}{p_{\prime}} = \frac{d_{\prime}}{l'} \, \frac{2 k}{i} \, (\tau - t);$$

or

$$p_{\prime} = \mathrm{P}_{\prime} \, 10^{-\frac{d_{\prime}}{l'} \frac{2k}{i} (\tau - t)} \quad . \quad . \quad . \quad . \quad (16.)$$

If we assume the pressure $\mathrm{P}_{\prime}$ at the elevation $h = \alpha \, \phi \, \tau$, where α cannot be greater than 1, the conditions to be fulfilled require that for each value of t,

$$\alpha \, \phi \, \tau . 10^{-\frac{d_{\prime}}{l'} \frac{2k}{i} (\tau - t)} \leqq \phi \, t;$$

or

$$\alpha \, \phi \, \tau . 10^{-\frac{d_{\prime}}{l'} \frac{2k}{i} \tau} \leqq \phi \, t . 10^{-\frac{d_{\prime}}{l'} \frac{2k}{i} t};$$

and if for $\phi \, \tau$, and $\phi \, t$, we substitute the expression (9.),

$$\alpha \, 10^{-\left(\frac{d_{\prime}}{l'} \frac{2k}{i} - a\right) \tau - c \tau^2} \leqq 10^{-\left(\frac{d_{\prime}}{l'} \frac{2k}{i} - a\right) t - c t^2}.$$

If we suppose t to decrease without limit with increasing elevation, it would attain a negative value, for which, even with the smallest value of α, the conditions would cease to be fulfilled;

but we must not hence conclude Dalton's assumption as not reconcilable, under all circumstances, with the existence in equilibrium of an atmosphere of aqueous vapour of which the density is always a positive quantity. The decrease of t does not go on indefinitely, but only as far as the value which it possesses at the limit of the atmosphere; the formula (9.), which expresses the condition, is merely an interpolation formula, and has no justification beyond its more or less satisfactory accordance with Dalton's experiments made between $t = 0$, and $t = 100°$.

If we take the logarithm of the two quantities, between which the conditions apply, it follows that

$$\text{lo}\,\alpha \underset{<}{=} \left(\frac{d_{\prime}}{l'} \cdot \frac{2\,k}{i} - a + c\,(\tau + t)\right)(\tau - t);$$

and we also know that $\alpha \underset{<}{=} 1$, so that log α must not be positive. Hence it follows that the conditions may be fulfilled, or that the atmosphere of aqueous vapour is possible; also that the value of α (< 1), which determines its density at the elevation h, remains arbitrary, if

$$\frac{d_{\prime}}{l'}\,\frac{2\,k}{i} > a - c\,(\tau + t) \quad . \quad . \quad . \quad . \quad (19.)$$

which must be the case up to the limit of the atmosphere; further, that in the opposite case, if even at the height h,

$$\frac{d_{\prime}}{l'}\,\frac{2\,k}{i} < a - c\,(\tau + t)\,. \quad . \quad . \quad . \quad (20.)$$

the existence of an atmosphere of aqueous vapour in equilibrium is possible; but its density, at the elevation h, is limited by the condition that α must be less than

$$10^{\left(\frac{d_{\prime}}{l'}\,\frac{2\,k}{i} - a + c(\tau + t)\right)(\tau - t)}\,. \quad . \quad . \quad (21.)$$

for the value of t at the limit of the atmosphere. In a *particular* case of the decrease of temperature, the atmosphere of aqueous vapour may be at all elevations as dense as the temperature permits; this case requires that

$$\frac{d\,p_{\prime}}{p_{\prime}} = -\frac{d_{\prime}}{\mu\,l'}\,\frac{d\,X}{1 + k\,t} = \frac{d\,\phi\,t}{\phi\,t};$$

or, under the supposition of the customary expression for $\phi\,t$, that

$$-\frac{d_{\prime}}{l'}\,d\mathrm{X} = (a-2\,c\,t)\,(1+k\,t)\,dt.$$

Hence follows by integration,

$$a\,(\tau-t)+\left(\frac{a\,k}{2}-c\right)(\tau^2-t^2)-\frac{2}{3}\,c\,k\,(\tau^3-t^3)=\frac{d_{\prime}}{l'}\,(\mathrm{X}-\mathrm{H})\,;$$

for which we may also write

$$\mathrm{X}-\mathrm{H}=\frac{l'}{d'}\Big\{(a-2\,c\,\tau)\,(1+k\,\tau)\,(\tau-t)$$
$$-\left(\frac{a\,k}{2}-c+2\,c\,k\,\tau\right)(\tau-t)^2-\frac{2}{3}\,c\,k\,(\tau-t)^3\Big\}\ .\ (22.)$$

If we introduce into this equation the values of l', $d_{\prime}$, a, c, k already employed, we obtain the law of the decrease of temperature, which, on Dalton's supposition, is alone reconcilable with an atmosphere everywhere saturated with aqueous vapours.

$$\mathrm{X}-\mathrm{H}=424^{\mathrm{T}}\!\cdot\!0\,(1-\tau\,.\,0{\cdot}00447)\,(1+\tau\,.\,0{\cdot}00375)\,(\tau-t)$$
$$+\,0^{\mathrm{T}}\!\cdot\!15\,(1-\tau\,.\,0{\cdot}0463)\,(\tau-t)^2-0^{\mathrm{T}}\!\cdot\!003\,(\tau-t)^3.$$

If, further, according to Sect. 2, we put

$$\frac{2\,k}{i}=\frac{\mathrm{H}'-\mathrm{H}}{\tau-\tau'}\cdot\frac{1}{1+k\mathrm{T}},$$

and designate by (t) the value of t at the extreme limit of the atmosphere, the condition (19.) becomes

$$\frac{\mathrm{H}'-\mathrm{H}}{\tau-\tau'}>\frac{l'}{d_{\prime}}\,(1+k\mathrm{T})\{a-c\,(\tau+(t))\}$$
$$>\{424^{\mathrm{T}}\!\cdot\!0-0^{\mathrm{T}}\!\cdot\!95\,(\tau+(t))\}\,(1+k\,\mathrm{T}).$$

The actual change of elevation, which produces a decrease of 1° in the height of the thermometer, is much less, or about = 85 toises; this is irreconcilable, under Dalton's supposition, with the saturation of the atmosphere with aqueous vapour at the surface of the earth. But if the condition (20.) be fulfilled, or if

$$\frac{\mathrm{H}'-\mathrm{H}}{\tau-\tau'}<(424^{\mathrm{T}}\!\cdot\!0-1{\cdot}9\,\tau)\,(1+k\mathrm{T})\,;$$

then, according to formula (21.), after substituting in it the expression for $\frac{2\,k}{i}$,

$$\log\alpha<\left\{\frac{\mathrm{H}'-\mathrm{H}}{\tau-\tau'}\ \frac{1}{1+k\mathrm{T}}\cdot\frac{d_{\prime}}{l'}-a+c\,(\tau+(t))\right\}(\tau-(t)),$$

or

$$(1+k\,\mathrm{T})\,\frac{l'}{d_{\prime}}\log\alpha < \left\{\frac{\mathrm{H}'-\mathrm{H}}{\tau-\tau'} - [424^{\mathrm{T}}\!\cdot\!0 - 0^{\mathrm{T}}\!\cdot\!95\,(\tau+(t))]\,(1+k\,\mathrm{T})\right\}(\tau-(t)),$$

whence follows

$$\tau-(t) < \frac{-\frac{l'}{d_{\prime}}\log\alpha\,.\,(1+k\,\mathrm{T})}{[424\cdot0-0\cdot95\,(\tau+(t))]\,(1+k\,\mathrm{T})-\frac{\mathrm{H}'-\mathrm{H}}{\tau-\tau'}}.$$

If, then, we know both the last members of the denominator and α, we can compute by this formula a value of $\tau-(t)$, which, continuing Dalton's supposition, exceeds the difference of temperature between the elevation h and the limit of the atmosphere. If we take, for example, $\frac{\mathrm{H}'-\mathrm{H}}{\tau-\tau'}=85$ toises and $\mathrm{T}=0$, and suppose the atmosphere at the surface of the earth to be half saturated with aqueous vapour, we obtain approximately $\tau-(t)<13^{\circ}\!\cdot\!5$, which is scarcely equivalent to the usual decrease of temperature in 1200 toises, not to speak of the limit of the atmosphere; if $\tau-(t)=n\,.\,13^{\circ}\!\cdot\!5$, the extreme value of $\alpha=\frac{1}{2}n$. Dalton's supposition is therefore only reconcilable with a *very* small quantity of aqueous vapour in the atmosphere, and not with that which really exists. If we could, therefore, regard as correct the pre-supposition of the equilibrium of the atmosphere on which we have proceeded, the presence of a considerable quantity of aqueous vapour in the atmosphere would be a conclusive argument against Dalton's supposition. But this equilibrium never really exists, and I am indebted to Professor Neumann for the remark, that the density of aqueous vapour ascending from the surface of the earth must be increased by the resistance opposed to it by the air.

8.

I come now to the examination of the supposition, that the temperature between two elevations at which it has been observed varies according to the law which has been assumed by Laplace, Sect. 2. The equation between t and X, which enounces this law, as deducible from Sect. 2, is

$$(1+k\,t)^2=(1+k\,\tau)^2\,\frac{\mathrm{H}'-\mathrm{X}}{\mathrm{H}'-\mathrm{H}}+(1+k\,\tau')^2\,\frac{\mathrm{X}-\mathrm{H}}{\mathrm{H}'-\mathrm{H}}\,.\quad (23.)$$

But we have no reason to regard as unreal moderate deviations,

in the transition of the temperature from τ to τ', from the rule prescribed by the equation. It remains, for instance, quite doubtful whether between the two elevations the true temperature may not differ from the value which would follow from the rule by a quantity amounting to one-tenth part of $\tau - \tau'$. It is not superfluous, therefore, to investigate further the influence of such possible deviations.

I will suppose that the true value of

$$\frac{1}{\mathrm{E}} = \frac{1 - 4\,\alpha\,k\,\dfrac{(\tau - t)\,(t - \tau')}{\tau - \tau'}}{1 + k\,t},$$

where t denotes the height of the thermometer at the elevation x, corresponding to equation (23.), and α is a constant coefficient, greater or less according to the amount of the deviation from the law. This expression of $\frac{1}{\mathrm{E}}$ is so chosen, that it agrees with the previous one at the two limits, and that the deviation of the temperature which it supposes attains its maximum $= \alpha\,(\tau - \tau')$ somewhere about $t = \frac{1}{2}\,(\tau + \tau')$ or $x = \frac{1}{2}\,(h + h')$. We obtain thence

$$\left(\frac{a}{a + x}\right)^2 \frac{d\,x}{\mathrm{E}} = -\,\frac{2\,k}{i}\,d\,t \left\{1 - 4\,\alpha\,k\,\frac{(\tau - t)\,(t - \tau')}{\tau - \tau'}\right\},$$

and the integral taken from h to h',

$$= \frac{2\,k}{i}\,(\tau - \tau') \left\{1 - \frac{2}{3}\,\alpha\,k\,(\tau - \tau')\right\}$$

$$= \frac{\mathrm{H}' - \mathrm{H}}{1 + k\,\mathrm{T}} \left\{1 - \frac{2}{3}\,\alpha\,k\,(\tau - \tau')\right\} \quad . \quad . \quad (24.).$$

It does not seem probable that in any case which is likely to occur the value of α would be comprehended within any very narrow limits, as for instance $\pm\,\frac{1}{10}$; if it should reach either of these limits, the consequent correction in the resulting difference of elevation, according to formula (23.), would be

$$= \pm\,\frac{\mathrm{H}' - \mathrm{H}}{1 + k\,\mathrm{T}} \cdot \frac{\tau - \tau'}{4000}.$$

So, for example, for a difference of elevation of 1000 toises, for which $\tau - \tau'$ is usually 12°, the correction would be about $\pm$ 3 toises. We should be the less inclined to assume that α must necessarily be very small, as it should not be overlooked

that the temperature of the air observed on a plain or on a height is always affected by the temperature of the surface of the earth. Hence we see, were it from this cause only, how little fitted barometrical measurements of height are to determine questions, the answers to which depend on *small* differences between theory and experiment. Possibly observations made late at night might agree better together than those made in the day when the surface of the earth is heated by the sun.

9.

It is known that Gay-Lussac found the value here denoted by $k = 0{\cdot}00375$, by experiments agreeing almost perfectly with each other; and that Dalton found exactly the same result from his experiments. The object of both these great physicists was to determine directly the increase which an unit of volume of dry air undergoes, when, the pressure remaining equal, the temperature is increased from freezing to boiling water. The accordance, not only of the several experiments in each series, but also of the results of the two series, has caused the determination of $k = 0{\cdot}00375$ to be generally regarded as one of the most certain that we possess: and there would be no reason for doubt respecting it at this period*, had it not been for recent experiments of Rudberg's, distinguished by the great care with which they were conducted, particularly in drying the air employed, and which give a considerably smaller value for k, i. e. 0·003648. Any later determination, contradicting an older one which has become in a degree classic by its intrinsic weight and by its general acceptance and use, ought to be accompanied by a strict examination of the older determination; and it is only when such criticism shows grounds for distrusting the older, that the more recent should be deemed deserving of preference. Rudberg has not entered into such a criticism. As the difference between the two values of k cannot be explained by the accidental errors of the experiments, as is shown by the

* I have myself determined, from my own observation, the value to be employed instead of k in computing astronomical refractions, and have found it 0·0036438; but this value must be different from that of k, and ought to be less, as shown in the 7th part of my observations, page xi. The research might have been spared had I possessed observations of the quantity of aqueous vapour in the air at the time of each observed refraction. It remedied the difficulty as far as could be done in the absence of a knowledge of the actual accidental state of the atmosphere on each occasion. But it is to be regarded as a contribution to the knowledge of astronomical refraction, and not as a determination of the value of k.

agreement of the partial results in the earlier as well as in the later series, it indicates a constant error, and there can be therefore no propriety in taking the arithmetical mean of the two determinations. I see no other course at present than to employ *both*, and to await a future decision on the differences which may result therefrom.

10.

Having gone through the different assumptions involved in formula (11.), I return to this formula, and will now show its application to barometrical measurements of height.

The pressures P and P′ at the elevations h and h', are deducible from the barometrical observations there made. If we denote one of the heights of the barometer by b, the temperature of the mercury and of the scale by which the height is measured by t, and assume that the scale is of brass as is usual, we obtain the mass of mercury supported by each unit of surface

$$= b \cdot \frac{53242 + t}{53242 + (t)} \cdot \frac{5550}{5550 + t},$$

where (t) signifies the normal temperature of the unit of measure of the barometer-scale, and where the unit of volume of mercury at the temperature of melting ice is taken as the unit of the mass. This mass presses in proportion to the force of gravity to which it is exposed; or with the force

$$(g) \left(\frac{a}{a + h}\right)^2,$$

and the pressure which it exerts is the product of both divided by the assumed unit of pressure ($=336^{l} \cdot 905$). Thus we obtain

$$\mathrm{P} = \frac{(g)\, b}{336 \cdot 905} \left(\frac{a}{a + h}\right)^2 \cdot \frac{53242 + t}{53242 + (t)} \cdot \frac{5550}{5550 + t};$$

and its Briggs's logarithm in formula 11, with sufficient approximation,

$$= \log b - \log \frac{336 \cdot 905\, (53242 + (t))}{53242} - \mu\, t \left\{\frac{1}{5550} - \frac{1}{53242}\right\} - \frac{2\mu}{a}\, \mathrm{H}.$$

If we put for a the geometrical mean of the two semi-diameters of the earth (log 6·5140838, *Ast. Nach.*, 333), and for (t) the normal temperature of the French standard foot $= 16^{\circ} \cdot 25$, then

$$\log P = \log b - \log 337{\cdot}008 - t \,.\, 0{\cdot}00007\dot{0}095 - \frac{H}{3760707}.$$

We obtain thus

$$\log \frac{P}{P'} = \log b_{,} - \log b'_{,} + \frac{H' - H}{3760707},$$

where $\log b_{,}$ and $\log b'_{,}$ are substituted for brevity for $\log b - t \,.\, 0{\cdot}00007$ and $\log b' - t' . 0{\cdot}00007$. Further, we obtain, without sensible error,

$$\sqrt{(P \cdot P')} = \frac{\sqrt{(b_{,} b'_{,})}}{337{\cdot}008}.$$

If we introduce $\log b_{,}$ and $\log b'_{,}$ in (11.), and put $\frac{l}{(g)}$ for l', this equation becomes

$$\log b_{,} - \log b'_{,} = \frac{(g)\,(H' - H)}{l\,(1 + k\,T)}$$

$$\left\{1 - \frac{l\,(1 + k\,T)}{(g) \,.\, 3760707} - \alpha \frac{0{\cdot}863}{\sqrt{(b'\,b'_{,})}}\, 10^{a\,T - c\,T^2}\right\}.$$

If we change (g) in the denominator of the second member into 1, which has no sensible influence, and if we take for α the half sum of its values at the two points of observation $= \frac{1}{2}(\alpha + \alpha')$, then the quantity within brackets is

$$= \frac{339{\cdot}17 - k\,T}{400{\cdot}17} \left\{1 - (\alpha + \alpha')\, \frac{172{\cdot}67}{\sqrt{(b_{,} b'_{,})}} \cdot \frac{10^{a\,T - c\,T^2}}{399{\cdot}17 - k\,T}\right\}.$$

If we designate thenceforward

$$l\,(1 + k\,T)\, \frac{400{\cdot}17}{399{\cdot}17 - k\,T} \quad . \quad . \quad . \quad . \quad \text{by } V,$$

$$\frac{172{\cdot}67 \,.\, 10^{a\,T - c\,T^2}}{399{\cdot}17 - k\,T} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad \text{by } W,$$

the equation gives

$$H' - H = \frac{\log b_{,} - \log b'_{,}}{(g)} \quad \frac{V}{1 - \dfrac{(\alpha + \alpha')\,W}{\sqrt{(b_{,} b'_{,})}}};$$

and as

$$H' - H = \frac{a\,h'}{a + h'} - \frac{a\,h}{a + h} = h' - h - \frac{h'^2}{a} + \frac{h^2}{a},$$

$$h' - h - \frac{h'^2}{a} + \frac{h^2}{a} = \frac{\log b_{,} - \log b'_{,}}{(g)} \quad \frac{V}{1 - \dfrac{(\alpha + \alpha')\,W}{\sqrt{(b'\,b'_{,})}}} \,. \quad (25.)$$

This is the most convenient form of the equation (11.) for use. It cannot be abbreviated further without giving up the power of bringing into the calculation the quantity of aqueous vapour contained in the atmosphere, as shown by the psychrometer at each station. The tables for facilitating the computation may also be so arranged, as to render quite inconsiderable the labour of taking the aqueous vapour properly into account.

11.

I will now explain the auxiliary tables. They are constructed logarithmically, like the small and very convenient tables of Gauss; but are rather more extensive, because they permit the result to be computed on either supposition of $k = 0{\cdot}00375$ or $= 0{\cdot}003648$, and also because they enable the influence of the aqueous vapour to be taken into account more completely than is done by the formula of Laplace.

If we denote

$$\log\{\log b_{\prime} - \log b_{\prime}'\} \text{ by B},$$

$$\frac{1}{1 - \dfrac{(\alpha + \alpha')\,\mathrm{W}}{\sqrt{(b_{\prime}\, b_{\prime}')}}} \quad . \quad . \quad \text{by V}',$$

$$\frac{1}{(g)} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad \text{by G},$$

then is the logarithm of $h' - h - \frac{h'^2}{a} + \frac{h^2}{a}$

$$= \mathrm{B} + \log \mathrm{V} + \log \mathrm{V}' + \log \mathrm{G}.$$

Table I. contains the value of

$$\log \mathrm{V} = \log \frac{9397{\cdot}74\,.\,400{\cdot}17\,(1 + k\,\mathrm{T})}{399{\cdot}17 - k\,\mathrm{T}}$$

calculated in the first column for $k = 0{\cdot}00375$, and in the last column for $k = 0{\cdot}003648$. Its argument is $2\,\mathrm{T} = \tau + \tau_{\prime}$. The second column contains

$$\log \mathrm{W} = \log \frac{172{\cdot}67\,.\,10^{a\mathrm{T} - c\mathrm{T}^2}}{399{\cdot}17 - k\,\mathrm{T}};$$

a single column is sufficient for log W, as the difference in the two values of k does not influence the last decimal. If we deduct from the tabular value of log W the half sum of the logarithms of $b_{\prime}$ and $b_{\prime}'$, the remainder is the logarithm of

$$\frac{(\alpha + \alpha')\,\mathrm{W}}{\sqrt{(b_{\prime}\, b_{\prime}')}},$$

supposing $\alpha = \alpha' = \frac{1}{2}$; if a different value be supposed for α and α', $\log(\alpha + \alpha')$ must be added. Hence is obtained the argument of Table II., which contains $\log V'$. Table III., with the argument $\varphi =$ the latitude, contains

$$\log G = \log \frac{1}{1 - 0{\cdot}0026257 \cos 2\varphi},$$

which formula rests on the value of the increase of the length of the seconds pendulum from the Equator to the Poles, deduced by Mr. Baily from the combination of all the known pendulum observations. Trans. Ast. Soc. vol. vii. page 94.

The sum of B and of the numbers taken from the three Tables, is $= \log \left\{ h' - h - \frac{h'}{a} + \frac{h^2}{a} \right\}$; to obtain from hence $h' - h$, we must add $\frac{h'^2}{a}$ and subtract $\frac{h^2}{a}$, which are both given by Table IV., which is to be entered with h' and with h.

It may be convenient to recapitulate the notation and rules:

b, b' are the heights of the barometer, read off on a scale divided into Parisian lines.

t, t' are the heights of the centesimal thermometer attached to the barometer.

τ, τ' are the heights of the centesimal thermometer in the free air.

α, α' are the degrees of saturation of the atmosphere with aqueous vapour.

The calculation of the difference of height of the points where these observations have been made, requires

1. $\log b_{,} = \log b - t \,.\, 0{\cdot}00007$; $\log b_{,}' = \log b' - t' \,.\, 0{\cdot}00007$.
2. $B = \log \{\log b_{,} - \log b_{,}'\}$.
3. $\log V$ and $\log W$, which, with the argument $\tau + \tau'$, are to be taken out of Table I.
4. $\log V'$, which is given in Table II., with the argument $\log \frac{(\alpha + \alpha')\,W}{\sqrt{(b_{,}\, b_{,}')}}$.
5. $\log G$, which is given in Table III., with the argument of the latitude $= \varphi$.
6. The log of the approximate difference of height $= B + \log V + \log V' + \log G$.

7. The true difference of height is the approximate, + the difference of the two small corrections which are obtained from Table IV., with the arguments of the greater and lesser height.

I take, as an example, one of D'Aubuisson's measurements of the height of Monte Gregorio, above a point at an elevation $h = 128{\cdot}3$ toises (Traité de Géognosie, i. p. 481.). There being no observation of the psychrometer, I take $\alpha = \alpha' = \frac{1}{2}$.

$$\begin{array}{lll} b = \overset{\mathrm{L}}{329{\cdot}013}, & t = \overset{\circ}{19{\cdot}85}, & \tau = \overset{\circ}{19{\cdot}95} \\ b' = 268{\cdot}215, & t' = 10{\cdot}5, & \tau' = \ \ 9{\cdot}9 \\ \log b = 2{\cdot}51721\,; & 7\,.\,t = 139\,; & \left.\begin{array}{l}\log b_{\prime} = 2{\cdot}51582 \\ \log b_{\prime}' = 2{\cdot}42774{\cdot}5\end{array}\right\}\text{half sum.*} \\ \log b' = 2{\cdot}42848\,; & 7\,.\,t' = \ \ 73{\cdot}5\,; & \\ & & \log \dfrac{b_{\prime}}{b_{\prime}'} = 0{\cdot}088075 \\ & & \mathrm{B} = 8{\cdot}94485 \end{array}$$

Table			
		$\mathrm{B} = 8{\cdot}94485$	
I.	$\tau + \tau' = 29°85$ ($k = 0{\cdot}00375$)	$\log\mathrm{V} = 3{\cdot}99782$	$\log\mathrm{W} = 0{\cdot}0397$
II.	Arg. $= 7{\cdot}5679$	$\log\mathrm{V}' = \quad 161$	* . . $2{\cdot}4718$
III.	$\phi = 45°32'$	$\log\mathrm{G} = \quad -2$	$7{\cdot}5679$
		$2{\cdot}94426$	
	Approximate height	$879^{\mathrm{T}}{\cdot}54$	
IV.	$h' = 1007{\cdot}8$, $h = 128{\cdot}3$	$+0\ {\cdot}31$	
		$h' - h = 879^{\mathrm{T}}{\cdot}85$	

D'Aubuisson himself computes the height $879^{\mathrm{T}}{\cdot}7$; from Gauss's tables we should have $879^{\mathrm{T}}{\cdot}63$. If k be taken $= 0{\cdot}003648$, we have $1^{\mathrm{T}}{\cdot}26$ less. If we take the air as dry, we obtain $3^{\mathrm{T}}{\cdot}24$ less; and if as saturated, $3^{\mathrm{T}}{\cdot}28$ more.

BESSEL.

TABLE I.

Argument = $\tau + \tau'$ (Centesimal scale.)

$\tau+\tau'$.	0·00375 log V.	log W.	0·003648 log V.	$\tau+\tau'$.	0·00375 log V.	log W.	0·003648 log V.
−20°	3·95747	9·3501	3·95793	20°	3·99014	9·9096	3·98971
−19	3·95832	9·3646	3·95875	21	3·99093	9·9229	3·99048
−18	3·95916	9·3792	3·95958	22	3·99171	9·9362	3·99124
−17	3·96001	9·3937	3·96040	23	3·99249	9·9495	3·99200
−16	3·96085	9·4083	3·96122	24	3·99328	9·9628	3·99277
−15	3·96169	9·4227	3·96203	25	3·99406	9·9760	3·99353
−14	3·96253	9·4372	3·96285	26	3·99484	9·9892	3·99428
−13	3·96337	9·4516	3·96366	27	3·99561	0·0023	3·99504
−12	3·96420	9·4660	3·96447	28	3·99639	0·0155	3·99580
−11	3·96504	9·4803	3·96529	29	3·99716	0·0285	3·99655
−10	3·96587	9 4946	3·96610	30	3·99794	0·0416	3·99731
− 9	3·96670	9·5089	3·96690	31	3·99871	0·0546	3·99806
− 8	3·96753	9·5232	3·96771	32	3·99948	0·0677	3·99881
− 7	3·96836	9·5374	3·96851	33	4·00025	0·0806	3·99956
− 6	3·96918	9·5516	5·96932	34	4·00102	0·0936	4·00031
− 5	3·97001	9·5657	3·97012	35	4·00179	0·1065	4·00106
− 4	3·97083	9·5799	3·97092	36	4·00255	0·1193	4·00180
− 3	3·97165	9·5940	3·97172	37	4·00332	0·1322	4·00255
− 2	3·97247	9·6080	3·97252	38	4·00408	0·1450	4·00329
− 1	3·97329	9·6221	3·97332	39	4·00484	0·1578	4·00403
0	3·97411	9·6361	3·97411	40	4·00560	0·1705	4·00477
+ 1	3·97493	9·6500	3·97490	41	4·00636	0·1833	4·00551
2	3·97574	9·6640	3·97570	42	4·00712	0·1960	4·00625
3	3·97655	9·6779	3·97649	43	4·00787	0·2086	4·00699
4	3·97736	9·6918	3·97728	44	4·00863	0 2212	4·00772
5	3·97817	9·7956	3·97806	45	4·00938	0·2338	4·00846
6	3·97898	9·7194	3·97885	46	4·01013	0·2464	4·00919
7	3·97979	9·7332	3·97963	47	4·01088	0·2589	4·00992
8	3·98059	9·7470	3·98042	48	4·01163	0·2714	4·01066
9	3·98140	9·7607	3·98120	49	4·01238	0·2839	4·01139
10	3·98220	9·7744	3·98198	50	4·01313	0·2963	4·01211
11	3·98300	9·7880	3·98276	51	4·01388	0·3087	4·01284
12	3·98380	9·8017	3·98354	52	4·01462	0·3211	4·01357
13	3·98460	9·8153	3·98431	53	4·01536	0·3335	4·01429
14	3·98539	9·8288	3·98509	54	4·01611	0·3458	4·01502
15	3·98619	9·8424	3·98586	55	4·01685	0·3581	4·01574
16	3·98698	9·8559	3·98663	56	4·01759	0·3703	4·01646
17	3·98777	9·8693	3·98741	57	4·01832	0·3824	4·01718
18	3·98856	9·8828	3·98818	58	4·01906	0·3946	4·01790
19	3·98935	9·8962	3·98894	59	4·01980	0·4068	4·01862
20	3·99014	9·9096	3·98971	60	4·02053	0·4189	4·01933

TABLE II.						TABLE III.				TABLE IV.	
Argument $= \frac{(\alpha + \alpha') W}{\sqrt{(b, b_{\prime})}}$.						Argument = Latitude.				Arg. Height $\begin{cases} h' + \\ h - \end{cases}$	
Arg.	log V′.	Arg.	log V′.	Arg.	log V′.	ϕ.	log G′.	ϕ.	log G′.	h' and h.	
						°				T	T
5·0	0	7·55	154	7·95	389	0	114	40	20	100	0·00
5·1	1	7·56	158	7·96	398	1	114	41	16	200	0·01
5·2	1	7·57	162	7·97	407	2	114	42	12	300	0·03
5·3	1	7·58	165	7·98	417	3	114	43	8	400	0·05
5·4	1	7·59	169	7·99	427	4	113	44	4	500	0·08
5·5	1	7·60	173	8·00	437	5	112	45	0	600	0·11
5·6	2	7·61	177	8·01	447	6	112	46	− 4	700	0·15
5·7	2	7·62	181	8·02	457	7	111	47	− 8	800	0·20
5·8	3	7·63	186	8·03	468	8	110	48	− 12	900	0·25
5·9	3	7·64	190	8·04	479	9	109	49	− 16	1000	0·31
6·0	4	7·65	194	8·05	490	10	107	50	− 20	1100	0·37
6·1	5	7·66	199	8·06	502	11	106	51	− 24	1200	0·44
6·2	7	7·67	204	8·07	513	12	104	52	− 28	1300	0·52
6·3	9	7·68	208	8·08	525	13	103	53	− 31	1400	0·60
6·4	11	7·69	213	8·09	538	14	101	54	− 35	1500	0·69
6·5	14	7·70	218	8·10	550	15	99	55	− 39	1600	0·78
6·6	17	7·71	223	8·11	563	16	97	56	− 43	1700	0·88
6·7	22	7·72	229	8·12	576	17	95	57	− 46	1800	0·99
6·8	27	7·73	234	8·13	590	18	92	58	− 50	1900	1·11
6·9	34	7·74	239	8·14	604	19	90	59	− 54	2000	1·22
7·0	43	7·75	245	8·15	618	20	87	60	− 57	2100	1·35
7·1	55	7·76	251	8·16	632	21	85	61	− 60	2200	1·48
7·2	69	7·77	256	8·17	647	22	82	62	− 64	2300	1·62
7·3	87	7·78	262	8·18	662	23	79	63	− 67	2400	1·76
7·4	109	7·79	269	8·19	678	24	76	64	− 70	2500	1·91
7·41	112	7·80	275	8·20	694	25	73	65	− 73	2600	2·07
7·42	114	7·81	281	8·21	710	26	70	66	− 76	2700	2·23
7·43	117	7·82	288	8·22	727	27	67	67	− 79	2800	2·40
7·44	120	7·83	295	8·23	744	28	64	68	− 82	2900	2·58
7·45	123	7·84	302	8·24	761	29	60	69	− 85	3000	2·76
7·46	125	7·85	309	8·25	779	30	57	70	− 87	3100	2·94
7·47	128	7·86	316	8·26	798	31	54	71	− 90	3200	3·13
7·48	131	7·87	323	8·27	816	32	50	72	− 92	3300	3·33
7·49	134	7·88	331	8·28	835	33	46	73	− 94	3400	3·54
7·50	138	7·89	338	8·29	855	34	43	74	− 97	3500	3·75
7·51	141	7·90	346	8·30	875	35	39	75	− 99		
7·52	144	7·91	354	8·31	896	36	35	76	−101		
7·53	147	7·92	363	8·32	917	37	31	77	−102		
7·54	151	7·93	371	8·33	939	38	28	78	−104		
7·55	154	7·94	380	8·34	961	39	24	79	−106		
		7·95	389	8·35	983	40	20	80	−107		

Article XVII.

On the Anhydrous Sulphate of Ammonia. By Heinrich Rose.

[From Poggendorff's *Annalen*, vol. xlix. p. 183.]

In attempting to precipitate the excess of sulphuric acid from a solution of anhydrous sulphate of ammonia by means of carbonate of barytes, I succeeded in obtaining crystals of considerable size from the fluid separated from the sulphate and excess of carbonate of barytes; these crystals I took for anhydrous sulphate of ammonia; having obtained only a small quantity I did not subject them to analysis, but employed for this purpose the indistinctly crystallized mass, which remained with these crystals after evaporation over sulphuric acid. I found in them only 67·47 per cent. of sulphuric acid instead of 70·03, which, according to theory, the anhydrous sulphate of ammonia should contain*.

I have since separated, in the above-described manner, the excess of acid from larger portions of the anhydrous sulphate of ammonia, and have obtained a greater quantity of these crystals. At the same time I investigated more accurately the action of water on this salt, which had been carefully prepared, and was perfectly neutral. After precipitating the excess of acid by carbonate of barytes, I satisfied myself that the solution had precisely the same properties as the salt obtained by treating anhydrous ammonia with anhydrous sulphuric acid. I found also, what I had not been before able to decide with certainty, on account of the small quantity of the salt employed, that the solutions do not contain one salt, but two different salts, possessing very singular properties, and remarkable as to their composition†.

I have also submitted the properties and composition of neutral anhydrous sulphate of ammonia to a fresh examination, and have ascertained some facts which will complete my former investigations. I have called this salt sulphat-ammon‡, for reasons formerly explained; the two salts obtained from its aqueous

* Poggendorff's *Annalen*, Bd. 47, S. 474.
† Poggendorff's *Annalen*, Bd. 32, S. 81.
‡ Ebendaselbst, Bd. 37, S. 475.

solution, I will, at present, denominate *parasulphat-ammon*, and the *deliquescent salt*; the names of sulphat and parasulphat-ammon are, however, to be considered as merely provisional; I shall very readily withdraw them if the ingenious views of Mr. R. Kane*, which regard ammonia as an amide of hydrogen, should be more generally adopted. It is indeed true, that, by this hypothesis, the phænomena which the compounds of anhydrous sulphuric acid with ammonia exhibit with reagents, are capable of more satisfactory explanation than by other theories; but as to the numerous compounds of ammonia with oxyacids and with water, the opinion of Berzelius, that these combinations contain the oxide of ammonium, is more simple and probable, because these salts, considered in this light, are analogous in composition to the salts of other bases.

I. *Neutral Anhydrous Sulphate of Ammonia—Sulphat-ammon.*

The principal properties of this compound I have described in a former paper, in which I especially mentioned its action on the solutions of barytic salts, oxide of lead, strontia and lime, and chloride of platina. Other reagents, which instantly indicate the presence of ammonia in a solution of sulphate of ammonia, do it imperfectly in a solution of sulphat-ammon. In order to determine this point, equal parts of sulphat-ammon and of sulphate of oxide of ammonium were dissolved each in nine times its weight of water, and both solutions were tested with the same reagents; sulphat-ammon is not perfectly soluble in less water than employed in this experiment.

A solution of sulphate of alumina soon produced crystals of alum in the solution of the sulphate of oxide of ammonium, but none were immediately produced in the solution of sulphat-ammon; after some time a small quantity was formed, but much less than in the sulphate of oxide of ammonium.

A concentrated solution of tartaric acid soon produced a crystalline precipitate with the sulphate of oxide of ammonium, and also after a longer time in the sulphat-ammon. A concentrated solution of racemic acid, which is a much more sensible test of ammonia than tartaric acid, produced similar effects; but the quantity of precipitate was much greater in the sulphate of oxide of ammonium.

* Researches on the Nature and Composition of the Compounds of Ammonia. Transactions of the Royal Irish Academy, vol. xix. p. 1.

A solution of carbazotic acid acted in the same way; it immediately produced a considerable precipitate with the sulphate of oxide of ammonium, and to a less extent, and after a longer time with the sulphat-ammon.

The sulphat-ammon is a homogeneous powder; when examined by the microscope it does not exhibit any appearance of crystallization; like other powders it attracts moisture from the air, but this is got rid of without any change of properties, by drying in a water-bath, and by fresh exposure it gains as much water as before.

Although I have already stated an analysis of sulphat-ammon, yet having, by a method which I shall hereafter describe, obtained it in larger quantity and of great purity, I have considered it necessary to repeat the examination. The proofs of the purity of this salt are not only that it scarcely reddens litmus paper, but on the contrary renders it blue (after it has been reddened), but only to a slight degree, and this effect it continues to produce only when kept in a bottle containing ammoniacal gas. When litmus paper, which has been dipped in a solution of sulphat-ammon, sulphate of oxide of ammonium, or most other soluble ammoniacal salts, is dried in the air, it is reddened.

One hundred parts of sulphat-ammon were treated with a solution of chloride of barium; the whole was evaporated to dryness, heated to low redness, treated with hydrochloric acid and water, and there were obtained 203·79 parts of sulphate of barytes. This is the only method by which the whole of the sulphuric acid can be converted into sulphate of barytes; but this substance, when so procured, passes through filters, and requires frequent filtration. The sulphate of barytes obtained indicates 70·04 of sulphuric acid, which agrees as nearly as possible with the amount of this acid calculated from the formula $\dddot{S} + \underline{N}\,\underline{H}^3$, or 70·03 per cent. The results of several analyses, confirming this composition, will be subsequently stated.

II. *Parasulphat-ammon.*

I have thus denominated a remarkable salt, which crystallizes in large well-formed crystals from the concentrated aqueous solution of sulphat-ammon; they may likewise be obtained by combining sulphat-ammon with anhydrous sulphuric acid by a method already mentioned. These are the crystals which

have been described by my brother in Poggendorff's *Annalen*, B. XLVII. 476. These crystals are obtained by evaporating the solution; but, like that of sulphate of oxide of ammonium, it is apt to become acid during the operation, and to have its properties thereby difficultly recognized; it is better to evaporate over sulphuric acid *in vacuo*. On further evaporating the mother-water another salt is formed, which differs essentially in its properties from the larger crystals; but it is difficult to separate it from them, especially when considerable quantities of the sulphat-ammon have not been operated upon. This salt attracts moisture from the atmosphere, which is not the case with the crystals of the parasulphat-ammon, this when quite dry suffering no alteration by exposure to the air. Of this salt I shall treat in the following section.

The parasulphat-ammon is rather more soluble than the sulphat-ammon; its solution is neutral to litmus paper. When also preserved for a long time, so that nothing can evaporate and crystallize, it remains neutral. When, however, the salt is moistened with water, it acquires in a short time the property of reddening litmus paper, and the solution possesses qualities and acts differently with reagents from that of the salt not previously moistened.

The acid reaction, which the salt acquires by moistening, probably arises from the expulsion of some ammonia by the water; the carbonic acid of the atmosphere appears also to exert some action; for if a solution of parasulphat-ammon is slowly evaporated, cold, over sulphuric acid, in contact with the air, it often acquires an acid reaction, which is not the case if the evaporation be performed *in vacuo*; when the crystals of this salt are obtained, no attempt must be made to free them from the solutions by washing with water; they must be dried only by blotting-paper.

What particularly characterizes the parasulphat-ammon, and distinguishes it from the sulphat-ammon is, that the solution of the dry salt is not rendered turbid by the salts of barytes or of lead, even when they remain long mixed. This property, it is, however, sometimes difficult to observe, partly because the crystals may contain a portion of the solution from which they have separated, and therefore contain the deliquescent salt; and partly from having been exposed to the atmosphere after moistening, and then yielding a solution which reddens litmus

paper; in both these cases the solutions instantly precipitate the salts of barytes and lead.

If hydrochloric acid and a solution of chloride of barium be added to one of parasulphat-ammon, it remains also for some time perfectly clear; in about twelve hours, however, a precipitate of sulphate of barytes is formed; but it does not occur without the hydrochloric acid be present.

In the property of not precipitating the solutions of barytic salts in the cold, the parasulphat-ammon very much resembles the compound obtained by M. Regnault, by saturating sulphate of chloride of sulphur $S\,\underline{Cl}^3 + 2\,\dddot{S}\,(\ddot{S}\,\underline{Cl})$ with anhydrous ammonia*, and which he considered as a mixture of sal-ammoniac and a sulfamide $(\ddot{S}\,\underline{N}\,\underline{H}^2)$. The solution of this compound occasions no precipitation with the salts of barytes, even when they have been long in contact. M. Regnault did not succeed in separating this sulfamide from sal-ammoniac by crystallization; and he adds, moreover, that the compound which he obtained very soon attracts moisture from the air, which, as already mentioned, is not the case with the crystals of parasulphat-ammon or sulphat-ammon.

The results of analyses prove, likewise, that the crystals cannot be regarded as an anhydrous sulfamide; 100 parts dissolved in water, were mixed with a solution of the chloride of barium and boiled. After some time a precipitate of sulphate of barytes appeared, but less in quantity and much more slowly than would have occurred, under similar circumstances, with a solution of sulphat-ammon. The whole was evaporated to dryness; the residue heated to incipient redness, left 203·64 parts of sulphate of barytes after treatment with hydrochloric acid and water; this is equivalent to 70 of sulphuric acid.

The result of this analysis proves that these crystals possess as exactly as possible the same composition as the anhydrous sulphate of ammonia or sulphat-ammon. If the sulphur in an anhydrous sulfamide $\ddot{S}\,\underline{N}\,\underline{H}^2$ was entirely converted into sulphuric acid, there would be obtained 80·03 per cent. of sulphuric acid from the sulfamide employed.

One hundred parts of crystals of parasulphat-ammon, which had been formerly prepared, gave, when treated in the same

* *Ann. de Chim. et de Phys.* lxix., 170.

manner, 204·49 parts of sulphate of barytes, equivalent to 70·29 of sulphuric acid. If we were to regard the crystals prepared by me, on account of their similarity to the combinations formed by M. Regnault, as a sulfamide, it must be considered as hydrated, $\ddot{S}\,\underline{N}\,\underline{H}^2 + \dot{\underline{H}}$.

Since, however, the existence of hydrous amides is not sufficiently proved, and even appears in some respects to be improbable, I have denominated these crystals *parasulphat-ammon*, or *parasulphammon*, on account of their similar per centage composition with sulphat-ammon.

In the solution of the parasulphat-ammon the ammonia is still more imperfectly separated by reagents than in a solution of the sulphat-ammon. In solutions of equal strength, one part of each salt to nine parts of water, a concentrated solution of tartaric acid does not effect the formation of supertartrate of ammonia, even after several days in the parasulphat-ammon, while a precipitate, though not an abundant one, is produced in the sulphat-ammon. A concentrated solution of racemic acid occasions, after some time, a very small quantity of crystalline precipitate in the solution of parasulphat-ammon, and much smaller than in the solution of sulphat-ammon; solutions of chloride of platina, carbazotic acid and sulphate of alumina, react in the same manner with the solution of sulphat-ammon.

As the presence of sulphuric acid is not indicated in the solution of parasulphat-ammon by the salts of barytes and lead, this is also the case, as might be anticipated, with the salts of strontia and of lime.

I have long hesitated whether the crystals of parasulphat-ammon should be regarded as distinct from the sulphat-ammon, merely on account of their different crystalline forms. It is well known how difficult it is to obtain perfectly anhydrous sulphuric acid; and, if it contain only a trace of water, a corresponding quantity of sulphate of ammonia is formed on saturation with dry ammoniacal gas; and the solution of barytes, being an extremely sensible reagent for sulphuric acid, it might easily happen that the solution of sulphat would be slightly precipitated even in the cold by barytes, owing to its being impure, on account of the presence of sulphate of ammonia. It is, indeed, true, that the solution of parasulphat-ammon acts somewhat differently from that of sulphat-ammon, with solutions of

barytes and lead and other reagents, and more particularly with tartaric and racemic acids; the sulphat-ammon is more sparingly soluble than the parasulphat, and does not so readily become acid when moistened; these, however, are circumstances of too little importance to allow of our regarding with certainty the parasulphat as a distinct substance from sulphat-ammon, and isomerical with it.

The following facts led me, however, to adopt this opinion: when a neutral solution of chloride of barium is added to a cold solution of pure sulphat-ammon, and the sulphate of barytes is allowed to precipitate for an hour, the filtered solution, without being heated again, deposits sulphate of barytes, and this occurs again after repeating the filtration; this is not the case with the parasulphat-ammon; its solution, after the addition of chloride of barium, remains for months perfectly clear in the cold, when no acid has been added; in performing these experiments equal portions of the isomerical salts were dissolved in similar quantities of water.

I consider these different actions as an essential difference between these substances; and the following series of experiments is also decidedly in favour of this difference: 100 parts of sulphat-ammon weighed 91·42 after drying in a water-bath; it was dissolved in cold water, without any acid, and mixed in the cold with a solution of chloride of barium; in an hour after mixing, the sulphate of barytes was separated by the filter and washed, towards the end of the operation, with warm-water; it weighed 51·71 parts, equivalent to 18·16 of sulphuric acid. Hydrochloric acid was added to the filtered solution, and it was evaporated to dryness; the residue, moderately heated, treated with water and a little hydrochloric acid, gave 145·7 of sulphate of barytes, equivalent to 51·16 of sulphuric acid; the whole quantity of sulphuric acid, therefore, in 100 parts, amounts to 69·32 parts, approximating very closely to the quantity contained in the sulphat-ammon by calculation.

In supposing that the 18·16 of sulphuric acid precipitated in the cold, might be derived from an admixture of sulphate of ammonia with the sulphat-ammon, they would be equivalent to 30·01 of the former salts, and the 51·16 of sulphuric acid obtained by evaporation indicate 73·08 of sulphat-ammon, giving an excess of 3·06, which the analyses will not admit of.

The results of two additional experiments are still more decisive of the difference between sulphat- and parasulphat-ammon; 100 parts of the same sulphat-ammon, as already employed, corresponding to 91·42 when dried in the water-bath, dissolved, cold and mixed with a solution of chloride of barium, gave 63·84 of sulphate of barytes, which was separated by the filter half an hour after precipitation, and are equivalent to 22·41 of sulphuric acid; in another experiment the sulphate of barytes separated an hour after precipitation, the sulphate obtained indicated 23·49 of sulphuric acid; the quantity of sulphate of barytes, obtainable by evaporation, was not determined in either experiment.

It is evident, from these experiments, that the quantities of sulphuric acid, precipitated in the cold by chloride of barium, may differ greatly; the three portions employed were weighed at the same time from the same quantity of the preparation; the greater or less quantity of the sulphate of barytes obtained in the cold, by a solution of the chloride of barium, undoubtedly depends not only upon how soon it is filtered, but upon the quantity of water in which the sulphat-ammon is dissolved, and the concentration of the solution of chloride of barium.

Were we to suppose, that in the last-described experiments, the sulphuric acid precipitated in the cold is derived from the sulphate of oxide of ammonium, there would arise greater contradictions than would attend the results of the first-mentioned analysis; for 22·41 parts of sulphuric acid would correspond to 37·03 parts of sulphate of oxide of ammonium. The different analyses of the sulphat-ammon having constantly given 70·03 per cent., or very nearly, of sulphuric acid, there would be obtained by further treatment 47·62 per cent. of the same acid, which corresponds to 68 parts of the sulphat-ammon. But in this case the quantities of sulphate of oxide of ammonium and the sulphat-ammon would amount to 105·03 per cent., and therefore the analyses would indicate an excess of 5·03 per cent.

In the last-mentioned examination of the sulphat-ammon 23·49 per cent. of sulphuric acid were obtained in the cold; if these indicated 38·81 parts of sulphate of oxide of ammonium, and if 46·54 parts of sulphuric acid, obtained by evaporating, correspond to 66·46 parts of the sulphat-ammon, the analyses would have given an excess of 5·27 per cent.

III. *The Deliquescent Salt.*

This salt, as already mentioned, is contained in the solution from which the parasulphat-ammon has crystallized; if this be evaporated to dryness over sulphuric acid *in vacuo*, imperfect crystals, or crystalline crusts only are obtained, which attract moisture from the air, and eventually deliquesce; it is very difficult to obtain this salt perfectly free from parasulphat-ammon; it is indeed more soluble, but the parasulphat is not very difficultly so, which renders it impossible to separate them when operating on small quantities; but in larger quantity I effected their separation in the following manner: I allowed the solution, which had been evaporated to dryness *in vacuo* over sulphuric acid partially to deliquesce by exposure to the air; or added a few drops of water to it, left them for some time in contact, and then evaporated the small portion of the salt [dissolved], again to dryness, as before, and employed it for analysis.

If the solution of the salt contains parasulphat-ammon, and if it has been evaporated very slowly over sulphuric acid, but not *in vacuo*, the crystals obtained from it become, in a moist state, very readily acid; the crystals of the parasulphat-ammon must therefore be picked out as much as possible from the mass evaporated to dryness, then the deliquescent salt must be dissolved in water, and carbonate of barytes added to the solution to saturate the free acid, and lastly, the solution must be again evaporated *in vacuo*.

The crystals of the salt are too indistinct to admit of their form being determined, and they are usually mere crystalline crusts, and any crystals which may be observed with bright faces are parasulphat-ammon.

The solution of this salt instantly precipitates solutions of barytes; but, as happens with the solution of sulphat-ammon, not nearly the whole of the sulphuric acid is thrown down in the state of sulphate of barytes. When hydrochloric acid is added to the solution, more sulphate of barytes is precipitated in the cold, than without such addition; a solution of chloride of strontium produces immediate precipitation in the solution of this salt only when very much concentrated; this distinguishes the solution from that of sulphat-ammon. If equal quantities of both salts are dissolved in similar quantities of water, both the solutions are not precipitated by a dilute solution of a salt

of strontium; after some time, however, if the solutions are not too dilute, precipitation begins in that of the deliquescent salt, while that of the sulphat-ammon remains clear. A solution of the acetate of peroxide [protoxide ?] of lead precipitates the solution of the deliquescent salt in the same way that it does the sulphat-ammon; a solution of chloride of calcium does not render either solution turbid; both solutions are similarly affected by chloride of platina, sulphate of alumina, tartaric acid, racemic acid, and carbazotic acid.

It is difficult to prevent the solution of the salt from reacting as an acid upon litmus paper, but it is inconsiderable if the salt has been carefully prepared.

The salt obtained by evaporating the solution *in vacuo* was dried at 212° until it ceased to lose weight; 100 parts of the dried salt dissolved in water, mixed with a solution of chloride of barium, and left in the cold for twenty-four hours, gave 20·42 of sulphate of barytes. Hydrochloric acid being added to the filtered solution, it was evaporated to dryness, and the residue was heated nearly to redness, and treated with hydrochloric acid. The quantity of the sulphate of barytes precipitated was 166·18 parts; the quantity of sulphate of barytes, precipitated in the cold, therefore, amounts to scarcely one-eighth of the whole; both quantities together gave 64·14 per cent. of sulphuric acid in the salt; this corresponds to a compound of anhydrous sulphate of oxide of ammonium, with half an atom of water, which, calculated according to the formula $\dddot{S}\,\underline{N}\ \underline{H}^{3} + \frac{1}{2}\,\underline{\dot{H}}$ gave in 100 parts

Sulphuric acid	64·93
Ammonia	27·79
Water	7·28
	100·

On repeating this experiment with a portion of the salt prepared on another occasion by dissolving pure sulphat-ammon, I obtained, by exposure to cold from 100 parts, after adding hydrochloric acid and chloride of barium to the solution, 106·06 parts of sulphate of barytes, and from the residue obtained by evaporation to dryness, and treating it with hydrochloric acid, 84·62 parts more of sulphate of barytes were obtained. It will be seen from these experiments that much more sulphuric acid

is precipitated from the salt when cold, if mixed with hydrochloric acid, than when this is not the case. The quantities of sulphate of barytes, added together, indicate 65·54 of sulphuric acid in the 100 of salt; the slight excess is unquestionably derived from the parasulphat-ammon which the salt contained, because it had been prepared from but a small quantity of the sulphat-ammon.

When I first prepared the crystals of the parasulphat-ammon, having obtained but a small portion of it, I resolved not to employ them for analysis, but to examine the irregularly crystalline masses obtained by evaporation to dryness, which must consist of a mixture of the deliquescent salt, and the parasulphat-ammon*; analyses confirmed this by finding only 67·47 per cent of sulphuric acid in this mixed substance. The hydrous sulphat-ammon is perfectly analogous to a salt which I obtained during my investigation of the compounds of carbonic acid and ammonia†, and which consists of carbonate of ammonia and half an atom of water, requisite to convert the ammonia [ammonium?] into the oxide of ammonium. The same is also the case with the hydrous sulphat-ammon. With respect to the carbonic salt, I have advanced the opinion that it might be regarded as carbonat-ammon with the carbonate of oxide of ammonium. The same view may also be adopted with respect to the hydrous sulphat-ammon, by regarding it as a compound of sulphat-ammon with the sulphate of oxide of ammonium $\dddot{S}\,\underline{N}\,\underline{H}^3 + \dddot{S}\,\underline{N}\,H^4$; the salt may perhaps also be formed by saturating the first hydrate of sulphuric acid $2\,\dddot{S} + \dot{\underline{H}}$, contained in Nordhausen sulphuric acid, with dry ammoniacal gas.

The deliquescent salt unquestionably originates from the parasulphat-ammonia when dissolved in water, and remaining for some time in contact with it. Very pure crystals of the parasulphat-ammon, quite free from the deliquescent salt, when dissolved in water, and evaporated over sulphurio acid *in vacuo*, always yield a considerable quantity of the deliquescent salt, along with the crystals of parasulphat-ammon. As crystals of the parasulphat-ammon become acid, when exposed to moist air for some time, it seemed to me interesting to inquire into the nature of the alteration which they undergo. Some ex-

* Poggendorff's *Annalen*, Bd. xlvii. S. 474.
† Poggendorff's *Annalen*, Bd. xlvi. S. 373.

ceedingly pure crystals of the salt were reduced to powder and moistened during several hours, by which the salt acquired an acid reaction, and it was then perfectly dried in a water-bath; 100 parts of the dry residue were dissolved in cold water; the solution reddened litmus paper, but not strongly, and it precipitated solution of chloride of barium. By the method frequently mentioned, I obtained 198·19 parts of sulphate of barytes equivalent to 68·13 of sulphuric acid; it follows from this result that the parasulphat-ammon, by moistening with water, is partially converted into the deliquescent salt. The acid reaction arises from the presence of free hydrate of sulphuric acid.

It results from these investigations, that, although the sulphat-ammon seems to dissolve in water without decomposition; yet, when the solution crystallizes, the crystals obtained, notwithstanding they are similar in composition to the sulphat-ammon, possess many properties which differ from it. In the solution of the sulphat-ammon the constituents of water are more readily combined with it by the action of certain reagents, and the compound therefore changes more readily. This is the case with the crystallized sulphat-ammon, or the parasulphat-ammon, which resists more powerfully the action of such reagents. The conditions of the sulphat-ammonand parasulphat-ammon, may be compared with the vitreous and crystalline state of certain bodies, in which they exhibit different properties.

The combinations of anhydrous sulphuric acid with ammonia may be regarded, according to Dr. Kane, as perfectly analogous to the hydrate of sulphuric acid. By supposing that ammonia is an amide of hydrogen, and that the amide combines in a similar manner with other bodies, as oxygen and chlorine, the amide of hydrogen becomes a body analogous to the oxide and chloride of hydrogen. But when sulphuric acid is combined with water or other oxibases, it may possess properties very different from those which belong to it when combined with the amide of hydrogen. We have, in fact, of late, become acquainted with a great number of cases, in which the sulphuric acid, when combined with certain substances, as for example, with the oxide of ethule, and other bodies of organic origin, loses some of the peculiarities by which we were previously accustomed to characterize it, especially that of giving an insoluble precipitate with barytic salts. But [hypothetical]

as this opinion may be, the explanation which Dr. Kane gives of the compounds of ammonia with hydrous oxacids is equally so; these are regarded by Berzelius as salts of the oxide of ammonium, and on this view, the analogy of these salts, with those formed with other oxibases, is maintained; as is also the isomorphism of some salts of potash and the oxide of ammonium; and these opinions were rapidly and almost universally adopted. But according to Dr. Kane, this numerous class of ammoniacal salts consists of combinations of acids with two bases, the oxide and amide of hydrogen; and the sulphate of oxide of ammonium becomes on this view analogous to several sulphates, which, at a higher temperature, retain one atom of water. But the perfect analogy and isomorphism of these ammoniacal salts with the salts of potash, are thrown into the back ground by Dr. Kane's theory, instead of being advanced. Prof. Graham*, for similar reasons, adopts the opinions of Berzelius justly, as he acknowledges the importance of the theory of Dr. Kane.

I will direct the attention of the reader to an analogy existing between the compounds of sulphuric acid with ammonia, and of the same acid with bicarburetted hydrogen (the elayl or ætherol of Berzelius) which was long since pointed out by Dumas†. The elayl and the ammonium produce, when combined with hydrogen, one the hypothetical radicle æthyle, the other the no less hypothetical radicle ammonium; both radicles may be combined with sulphur, chlorine, bromine and iodine: combined with the elements of water, one yields the base, oxide of æthyle, the other the base oxide of ammonium. Both bases may be combined with anhydrous oxyacids; both the bicarburetted hydrogen, as well as the ammonia, may be united by direct combination with anhydrous sulphuric acid; this acid may likewise be combined with oxide of æthyle, a compound contained in the sulpho-tartaric acid and in its salts, and also with the oxide of ammonium. The sulphuric acid forms compounds also with elayl (or rather with ætherol), as well as with ammonia, which contain so much water, or its elements, that only half the quantity of the bicarburetted hydrogen or the ammonia can be converted by it into the oxide of æthyle, or the oxide of ammonium; the former compound is the oil of wine (sulphate of the oxide of

* Elements of Chemistry. By T. Graham, p. 117.

† Poggendorff's *Annalen*, Bd. xii. S. 452.

æthyle—ætherol), the latter the deliquescent salt, contained in the mother-water, from which the parasulphat-ammon is separated by crystallization.

Much value, however, is not to be attached to these comparisons, for they merely refer to a certain analogy or combination, which may be even called a remote one, since bicarburetted hydrogen and ammonia differ with respect to the number of their elements.

This parallel becomes still more improbable, on account of the different properties of the substances compared, they possessing not the least resemblance to each other.

Article XVIII.

On a Transportable Magnetometer. *By* Wilhelm Weber.

[This article is translated partly from the *Resultate aus den Beobachtungen des magnetischen Vereins im Jahre* 1838, and partly from manuscript communications from M. Weber to Major Sabine.]

A SMALL travelling apparatus for the absolute measurement of the force of the earth's magnetism has been described in the *Resultate* for 1836*. That apparatus was not a magnetometer, but rather served as an illustration of the mode in which this measurement, which had previously been executed only with a magnetometer, might be made with an ordinary compass needle.

The degree of accuracy attainable with such a small apparatus, and the occasions on which it ought to be employed, were examined in the memoir referred to. But for the limitation imposed by the want of time, or by other external circumstances, it would of course be always preferable to use the magnetometer; the small apparatus being only intended to serve as a substitute, on occasions when the use of the more perfect instrument is impracticable. It is very desirable to reduce the number of such occasions as much as possible, by devising means of removing the difficulties which often oppose themselves to the use of the magnetometer; and this will appear the more desirable, the more we consider the great difference in the degree of precision attainable by the two instruments; and the more we reflect on the importance that would be given to a class of observations in which magnetometers have not hitherto been used, (namely, those made during distant and extensive journeys and voyages,) if they could be rendered susceptible of a higher degree of accuracy, certainty, and completeness.

If the final aim of such observations were simply that of constructing magnetic maps on which no ulterior investigation was to be based, the degree of exactness to which such maps should be carried might be arbitrarily determined; and possibly such an amount of accuracy as can be obtained without

* Translated in the Scientific Memoirs, Part V.

the use of magnetometers might be deemed sufficient. But if these maps are not themselves the final object sought,—if they are to form the basis of a new investigation,—if determinate rules and laws are to be recognised,—if the maps are to serve as the means of comparing experiment with the general theory of the earth's magnetism,—and if the elements of the theory are to be deduced from them,—then the degree of accuracy to be demanded is no longer arbitrary, but is determined by the nature of the subject. A *minor* degree of accuracy, such as these maps *now* possess, has, it is true, served for a first attempt at such a comparison; but in order that they may afford an adequate basis for an amended calculation, they must receive a *higher* degree of exactness. Such is now the great purpose of the magnetic observations to be made in distant expeditions, and it is this which now gives to such expeditions peculiar importance and value.

But the greater the importance which thus attaches to such voyages and observations, in consequence of the demands of theory, the more essential it becomes to examine what they are capable of affording.

Magnetic observations may be made at places widely remote from each other, either at the same time or nearly so, or alternately, so as to lessen the errors occasioned by regarding them as simultaneous. At all the stations, or at the more important at any rate, the observations may be continued with regularity for at least one or more weeks, so as to afford mean values freed in some measure from disturbing influences. But it is still more desirable to give to such expeditions the advantage of the recent improvements, by furnishing them with *magnetometers*. This would probably be best accomplished, by the persons who undertake magnetic expeditions making themselves thoroughly acquainted, both theoretically and practically, with the whole subject of magnetometric measurements, as they would then be able to devise for themselves the best travelling arrangements. But as there are not many opportunities of acquiring this knowledge, the following memoir may be interesting and useful to persons who cannot study the subject more thoroughly in other ways.

I proceed to describe a *transportable magnetometer*, which, as it unites all the advantages proper to magnetometers, with facility of management and compendious construction, appears

well adapted for magnetic expeditions and journeys, and is not more inferior to the magnetometers of fixed observatories, than good portable astronomical instruments are to the larger ones used in fixed astronomical observatories. I shall first give some general remarks on this instrument; then a description of its several parts; and lastly, observations of the Declination, and its Variations, made simultaneously with the transportable magnetometer and with that of the Göttingen Observatory, and a measurement of the Intensity made for the purpose of exhibiting its capability in that respect.

§ I. *General Remarks.*

The transportable magnetometer, figured in half size in Pl. XXV., fig. 1, requires in general but few explanations, as it is only essentially distinguished from other magnetometers by its small size, and by its more compendious construction. All the observations which are made with the larger magnetometers may also be made with the one under consideration; so that the *absolute declination*, the *variations of the declination*, and the *absolute horizontal intensity*, can all be measured by it; the *variations of the horizontal intensity* can also be observed, by suspending the bar employed in the experiments of deflection, as a bifilar magnetometer. The exactness with which these various measurements can be made is much greater than has yet been attained in travelling observations; it suffices for all the purposes of magnetic travellers; and it admits of as much accuracy and certainty, in proportion to its size, as do the largest magnetometers.

The results obtained with the large instrument used in the Göttingen Magnetic Observatory may be depended upon almost to the immediate readings, which are to $\frac{1}{10}$ of a division of the scale, or to 2 seconds of arc. This supposes the scale to be at least five meters from the mirror of the magnetometer, as otherwise the arc value of the divisions of the scale (which are one millimeter long), would be greater. Such a distance would not answer in journeys, as much time would be lost in bringing all the parts of the instrument into their proper positions. For travelling purposes, the distances ought to be limited so as to admit of the whole apparatus being placed on a table, and they should therefore be about four times less. Consequently, in lieu of the 8-inch theodolite, which is required to do full justice

to the great magnetometers, one of about three or four inches may be used without disadvantage, being at once more convenient and more œconomical, and still allowing the measurements to be depended upon to within from 10 to 20 seconds of arc. In considering the subject further, it will be seen, that admitting the necessity in the travelling apparatus of diminishing the observation distance, a diminution in the size of the magnetometer (which would not be admissible under other circumstances), does in no degree detract from the accuracy of the observations. For with a distance four times less, the degree to which the reading can be depended on (and which it is desired to preserve), is not affected, though the proportion of the magnetic force of the magnetometer to external disturbing influences be lessened in the same proportion. It may be assumed, that the magnetic force decreases as the cube of the linear dimensions of the bar, and external disturbing influences as the square, whence it follows that the bar may be made four times less without diminishing the dependence to be placed on the readings (which is to about the one tenth part of one division of the scale). If, with this diminution, other arrangements are adopted for guarding against external disturbing influences more carefully than has been hitherto found necessary with the larger magnetometers, there will be no material disadvantage in pushing the diminution in size somewhat further, having in such case only to preserve the degree of dependence which may be placed on the readings. In fact, the length of the bar of 600 millimeters has been reduced to 100 millimeters; and observation has shown that the readings may be equally depended upon; with this difference only, that the divisions, as read off, have a four times greater value of arc than in the case of the larger magnetometers, so that one division of the scale is equivalent to 80 seconds of arc instead of 20 seconds. Hence it appears, that by suitable arrangements, all the advantages of the *magnetometer* may be secured to *magnetic expeditions*; of course, without that highest degree of precision attainable only in fixed observatories, where nothing is wanting in construction and arrangement.

The instrument to be described affords these advantages in respect to the *absolute declination* and *its variations*, and still more in respect to the *absolute measurement of the horizontal intensity*; for in the *Resultate* for 1836, p. 88, it has been

shown*, that if both bars are six times smaller, the deflecting bar may be brought six times nearer to the magnetometer, without its being necessary to take more exactly into account the distribution of free magnetism in the bars. If, then, the length and breadth be diminished, and the thickness be left unaltered, (the large bars are 600^{mm} long, 36^{mm} broad, and 9^{mm} thick; and the small bars 100^{mm} long, 9^{mm} broad, and 9^{mm} thick,) it follows that as much may be gained in the small magnetometer, by increasing the angular deflection, as is lost by diminishing the distance of observation. In fact, the *experiments of deflection* admit of a precision which leaves nothing to be desired, and which harmonizes perfectly with the degree of accuracy which is known to be of easy attainment in the *experiments of vibration.*

Of course the small magnetometer must be constructed in such a manner that all its parts may form a solid whole, so that their relative position may not be liable to be disarranged by packing, unpacking, or putting up. It must be possible both to set the magnet bar at liberty, and to secure it again while in its case, as is done in the common compass, and the torsion of the thread must not be altered in so doing; the access of air must be quite cut off even from the mirror, which may be observed through a thin plate of mica, if a piece of plane glass ground parallel is not to be obtained. It is very advantageous to make the case entirely of copper, and even of strong copper-plates, not only for the sake of the increased solidity given to the whole apparatus, but also because the case will thus act on the inclosed magnet as a damper, and all the measurements may be made with much greater rapidity. The instrument must be so strong and solid, even when used in the open air, that it may carry two arms, which serve for placing the deflecting bar at equal measured distances east and west. These arms being correctly placed, all the preparations for the experiments of deflection which would otherwise be necessary,—namely, placing the measuring bars horizontally, and in a direction perpendicular to the magnetic meridian, and finding the corresponding points on either side of the magnetometer,—are spared, and the experiments are rendered much easier, and require less time.

* Sci. Mem. Part V. page 86.

§ II. *Description of the several parts.*

Fig. 1 represents the vertical section of the magnetometer in the direction of the magnetic meridian.

The magnetic bar which forms the needle is bored throughout its length, and the opening which is turned towards the telescope is provided with a lens, in the focus of which at the other end there is a cross of wires. This cross of wires is seen in the telescope, when (as is required for determining the true azimuth in the measurement of the absolute declination) it is adjusted to distant objects, and then directed to the lens. This arrangement was proposed by Airy, to make it possible to dispense with the mirror, and to be able to make, with the same telescope, and without displacing the eye-glass, the astronomical, geodesical, and magnetical observations required in measuring the *absolute* declination. In making this measurement the needle must be *reversed*; but in the reversal the optical axis must not alter its relative position in respect to the needle; this is effected in the closed case by means of a key, turned on the outside, and causing the needle inside to perform half a revolution round its axis of length. But this arrangement is inapplicable to observations which require *great changes* in the position of the needle, as in the experiments of vibration and deflection in the measurement of the absolute intensity. It therefore appeared advantageous to employ also a mirror, placed in the same manner as in the bifilar magnetometer, close to the axis of rotation of the needle, and above the copper case, and available however great the deflections may be.

The copper case is seen to have three openings: the first is into a space containing the mirror, and closed towards the theodolite by a plate of glass, through which the light can pass, in the direction shown in the plate from the scale, to the mirror, and thence back to the telescope of the theodolite. The other two openings are nearly at the same height as the magnetic needle and the telescope of the theodolite. The light entering through one of these apertures illuminates the cross of wires which is stretched across the hindmost end of the hollow needle, passes on to the lens at the other end, and thence, parallel to the horizontal direction marked in the figure, to the telescope of the theodolite with which the cross of wires is observed. The needle, bored throughout its length, is made accurately cylin-

drical, and is inclosed in a cylindrical brass box, on the under surface of which are two small projections which fit into two cavities in the copper case when the suspension thread of the needle is let down. The brass box can be fixed in this position by two screws brought through the upper part of the copper case: the box being thus held fast, the needle may *first* be drawn out through the opening in the back of the case, and a brass cylinder of the same form as the needle, inclosing a weak magnet, may be placed in its stead, to try the torsion of the thread. *Secondly*, for the purpose of measuring the error of collimation, the needle may be turned in the box round its longitudinal axis, by means of a key introduced through the aperture in the back of the case. During the observations the apertures in the front and the back of the case are closed with a plate of mica to guard against currents of air.

Fig. 2 represents a somewhat different and more simple construction of the same instrument; the needle is not hollow, is not enclosed in a brass case, and cannot be reversed. This simplification is admissible when the use of the instrument is to be restricted to the experiments which are to be made *in the open air*, as detailed in the sequel. In this case the mirror is included in the copper case, and its normal forms a right angle with the magnetic axis of the needle. The glazed opening in the side of the case does not impair its action as a damper, and the opening may be made of any convenient size.

Fig. 3 represents the outside box, in which the instrument is packed for travelling, and which serves also for suspending the deflecting bar when it is to be used for the experiments of vibration. A mirror is fixed to the end of the bar, so that it may be observed from a distance with a telescope and scale. The box has a small opening which can be closed with a plate of mica admitting the light. The figure shows the bar suspended in the box, and loaded with two cylindrical weights, made of brass, and connected by a silk thread passing over a bar parallel to the needle, to keep the centres of gravity of the two weights exactly the length of the bar from each other. The weights serve for the deduction of the moment of inertia.

The unifilar suspension of the bar can be changed for a bifilar, if the variations of the intensity are to be observed. The box must then be placed relatively to the theodolite and to the magnetometer in the manner represented in the ground plan, fig. 4, namely, so that, according to the rule laid down in the *Resultate*

for 1837, p. 22*, the line connecting the middle of the bar with the middle of the magnetometer needle may form with the magnetic meridian an angle of 35° 16′. Thus observations of the variations of the declination and of the intensity may be conveniently combined in this manner by travelling observers.

§ III. *Examples of Observations and Measurements.* *Measurement of the Absolute Declination.*

This measurement resolves itself into three parts: 1. The determination of torsion. 2. The azimuthal determination of the magnetic axis. 3. The azimuthal determination of the true meridian. By *the azimuth of a direction* is here understood the angle formed by two vertical planes, one in the direction in question, and the other in the direction of the optical axis of the telescope of the theodolite, the alidade being placed on the zero point of the circle.

1. *Determination of Torsion.*

This determination consists of the measurement of the *force* of torsion, and of the *angle* of torsion.

Force of Torsion.

There belong to the magnetometer two needles, the magnetic and the torsion needle, which may be suspended to the same thread, and which differ in the proportion of their magnetic moments (M, m). Designating by T the horizontal part of the earth's magnetic force, the force of torsion is to be compared with the force M T, as well as with the force m T.

Comparison with the force M T.

In order to reduce the observations to the same time, the declination was observed in the magnetic observatory simultaneously with both the observations.

Reading of the Torsion Circle.	Observation of the position of the Magnetometer by the Scale.	Observation in the Magnetic Observatory.	Radius in parts of the Scale.	Reduced Observation.
° ′		° ′ ″		
355 6	275·67	18 29 49	2174	275·67
175 6	237·06	18 30 42		237·31

Hence the force of torsion is given in parts of M T

$$= \frac{57{\cdot}295}{180°} \cdot \frac{38{\cdot}36}{2174} = \frac{1}{178}.$$

* Sci. Mem. Part VI. p. 270.

Comparison with the force m T.

Reading of the Torsion Circle.	Observation of the position of the Magnetometer by the Scale.	Differences.	Mean.	Radius in divisions of the Scale.
° ′				
269 15	270·77			
329 54	109·79	160·98		
269 15	280·91	171·12	167·69	2243·5
329 54	112·18	168·73		
269 15	282·12	169·94		

Hence the force of torsion is given in parts of m T

$$= \frac{57{\cdot}295}{60{\cdot}65} \cdot \frac{167{\cdot}69}{2243} = \frac{12{\cdot}563}{178}.$$

Angle of Torsion.

	Observation of the position of the Magnetometer by the Scale.	Radius in divisions of the Scale.
Magnetic needle..	292·90	2174
Torsion needle ..	328·67	

The distances of the observed divisions of the scale from the zero point of torsion being designated by x and y, then x is the angle of torsion sought, expressed in divisions of the scale; and for determining x we have the following equations:

$$292{\cdot}90 - x = 328{\cdot}67 - y$$
$$12{\cdot}563\, x = y.$$

Hence the angle of torsion in divisions of the scale is found,

$$x = 3{\cdot}09,$$

in seconds of arc

$$x = \frac{3{\cdot}09}{2174} \cdot 206265'' = 293''.$$

From this determination of the force and of the angle of torsion, the correction on account of torsion to be applied in measuring the declination is found

$$= \frac{1}{178} \cdot 293'' = 1''{\cdot}65.$$

This correction is so small that it may be wholly neglected; the more so, as, during the time occupied in the measurement, the declination itself altered two divisions of the scale, so that the angle of torsion for the time of this measurement almost wholly disappeared.

2. *Azimuthal determination of the Magnetic Axis.*

In order to reduce the observations to the same time, the declination was observed simultaneously in the magnetic observatory.

	Time. 1839, April 11.	Azimuth of the Collimation Line.	Observation in the Magnetic Observatory.	Reduced Azimuth.	Azimuth of the Magnetic Axis.
	h. m.	° ′ ″	° ′ ″	° ′ ″	° ′ ″
Before reversal	11 0	131 22 43	18 26 26	131 20 0	131 41 29·5
After reversal	11 37·5	132 2 59	18 29 9	132 2 59	

3. *Azimuthal determination of the true North.*

Three visible objects were observed, the positions of which, in respect to the Göttingen Observatory, are given by geodesical measurements.

Designation of the Objects.	Distance from the Observatory. South.	Distance from the Observatory. West.	Observed Azimuth.	Azimuth of the true North.
			° ′ ″	° ′ ″
Hohehagen ..	+ 6060·00	+ 12447·70	33 58 50	150 6 14
Gartenhaus ..	+ 289·28	− 27·54	315 17 5	
Jacobithurm..	− 710·70	+ 500·49	117 15 15	

As there is no correction to be applied on account of torsion, we obtain immediately from hence the *westerly declination*, by deducting the azimuth of the magnetic axis from the azimuth of the true north.

$$150^\circ\ 6'\ 14'' - 131^\circ\ 41'\ 29''\cdot 5 = 18^\circ\ 24'\ 44''\cdot 5.$$

This result corresponds to $11^{h}\ 37^{m}\cdot 5$, 11th April 1839. The declination observed at the same time in the magnetic observatory was

$$18^\circ\ 29'\ 9'',$$

showing a difference of $-4'\ 24''\cdot 5$, which probably is only in part due to error of observation, and is in part caused by the influence of the copper case surrounding the magnetometer, which may not be wholly free from iron. Repeated measurements, and comparisons with the observations in the magnetic observatory, may serve to deduce such an influence if it exists, so that it may be taken into account in future measurements. A second measurement actually gave a similar result, namely,

1839, April 13.	In the open air.	In the magnetic observatory.
$10^h\ 31'$	$18°\ 18'\ 0''$	$18°\ 23'\ 36''$

showing a difference of $-5'\ 36''$. The mean influence of the copper case in this instrument may therefore be taken as $=-5'$.

Observation of the Variations of Declination.

On the 15th April 1839, from $5^h\ 25^m$ to $7^h\ 27^m{\cdot}5$, the variations of declination were observed alternately, with the magnetometer in the Göttingen Observatory, and with the small magnetometer. In the following table the four first columns show the immediate results of observation with the two apparatus. In the final column the observations with the small magnetometer are reduced according to the proportion of the value of the scale divisions. The two series of observations are exhibited graphically in fig. 5, for the purpose of comparison. It may be seen from this example that the observations of the variations of declination can be made with a portable magnetometer with much accuracy.

1839. April 13.	Magnetic Observatory. A.	1839. April 13.	Transportable Magnetometer.	
			Reading x	Reduced Value $B = 895 + 3{\cdot}25\,(x - 244{\cdot}2)$
h. m.		h. m.		
5 25	896·00	5 27·5	244·95	897·44
5 30	895·56	5 32·5	244·20	895·00
5 35	894·66	5 37·5	244·97	897·50
5 40	896·47	5 42·5	245·20	898·25
5 45	899·56	5 47·5	246·18	901·44
5 50	899·52	5 52·5	245·78	900·14
5 55	898·78	5 57·5	246·02	900·91
6 0	900·57	6 2·5	247·35	905·24
6 5	905·95	6 7·5	248·04	907·48
6 10	908·00	6 12·5	249·77	913·10
6 15	916·77	6 17·5	251·77	919·60
6 20	920·00	6 22·5	251·77	919·60
6 25	919·66	6 27·5	251·56	918·92
6 30	916·63	6 32·5	250·70	916·12
6 35	912·72	6 37·5	250·96	916·97
6 40	917·66	6 42·5	251·74	919·51
6 45	927·35	6 47·5	254·32	927·89
7 0	941·27	7 2·5	260·79	948·92
7 5	959·33	7 7·5	265·71	964·91
7 10	964·53	7 12·5	261·27	950·48
7 15	936·38	7 17·5	254·34	927·95
7 20	922·80	7 22·5	251·75	919·54
7 25	914·42	7 27·5	250·09	914·14

Absolute Measure of the Intensity.

The measurement of the intensity divides itself into four parts. 1. The determination of torsion. 2. Of the moment of inertia of the deflecting bar. 3. The experiments of deflection. 4. The experiments of vibration. I will confine myself in this place, for the sake of brevity, to two parts, viz. the determination of the moment of inertia, and the experiments of deflection, which are especially instructive towards a knowledge of the instrument. The determination of torsion has been already spoken of in the measurement of declination, and the experiments of vibration are so simple and so well known, that it is sufficient to give their results.

1. *Determination of the Moment of Inertia.*

The deflecting bar is suspended to a thread or wire, and is then vibrated: 1) without a weight; 2) with a weight, the moment of inertia of which is known.

Vibrations without a weight.						
Number of Vibrations.	Time.			Arc of Vibration.		Reduced time of Vibration.
	h.	m.	s.	°	′	″
0	7	20	51·27	8	56	
26	7	23	45·49	8	40	6·698
61	7	27	39·92	8	8	6·695
115	7	33	41·64	7	22	6·696
151	7	37	42·80	6	56	6·695
186	7	41	37·19	6	32	
Vibrations with a weight.						
0	2	18	35·57	8	16	12·058
46	2	27	50·45	6	58	12·039
125	2	43	41·76	5	4	12·019
200	2	58	43·31	3	20	

Hence the mean time of vibration without a weight is $= 6''\cdot696$, and with a weight $= 12''\cdot039$. For determining the moment of inertia of the weight we have the following data: 1) the length l of the deflecting bar, or the distance apart of the threads which hang from its two ends and support two equal cylindrical weights; 2) the mass $2\,p$; 3) the radius r of the two cylinders.

$$l = 93^{\text{mm}}\cdot42$$
$$2\,p = 50000^{\text{mg}}$$
$$r = 4^{\text{mm}}\cdot60$$

If the mass of the cylinder were concentrated in its axis, its moment of inertia would be

$$\tfrac{1}{2}\, l^2\, p = 109091000.$$

If the cylinders revolved only round their own axis, their moment would be

$$r^2\, p = 529000.$$

Their moment in the above experiments is to be taken as equal to the sum of

$$\tfrac{1}{2}\, l^2\, p + r^2\, p = 109620000.$$

Whence therefore the moment of inertia of the oscillating bar may be obtained from the equation

$$\mathrm{M\,T} = \frac{\pi^2\,\mathrm{K}}{t^2} = \frac{\pi^2\,(\mathrm{K} + \mathrm{K}')}{t'^2},$$

where K' signifies the known, and K the desired moment of inertia, t' the time of vibration with a weight, and t the time of vibration without a weight, consequently

$$\mathrm{K} = 49103000.$$

In these experiments the needle was suspended to a thread in which the force of torsion was so small as to be insensible. The same series of experiments was repeated with the needle suspended by a wire in which the force of torsion was much greater; the result was almost the same as before, namely,

$$\mathrm{K} = 49044000.$$

Finally, in order to furnish a check, the deflecting bar was weighed, and its length and radius were exactly measured:

Weight	$p' =$	66670^{mg}
Length	$l =$	$93^{\mathrm{mm}}\!\cdot\!42$
Radius	$r' =$	$5^{\mathrm{mm}}\!\cdot\!45$,

whence its moment of inertia may be calculated. Supposing perfect internal homogeneity,

$$\mathrm{K} = \tfrac{1}{12}\, l^2\, p' + \tfrac{1}{4}\, r'^2\, p' = 48982000.$$

The accordance of all these experiments sufficiently shows that the moment of inertia of even such small bars may be determined with great precision.

2. *Experiments of Deflection.*

1839, February 13.			Double Deflection.		
Distance in Millimeters.	North Pole.	Readings.	In divisions of the scale.		Arc values.
− 556·75	E. W. E.	372·95 132·33 373·78	240·62 241·45	241·03	5° 30′·3
− 453·25	E. W. E.	475·91 28·36 476·58	447·55 448·22	447·89	10° 9′·3
+ 453·25	E. W. E.	480·04 31·83 480·27	448·21 448·44	448·32	10° 11′·2
+ 556·75	E. W. E.	375·93 135·06 375·82	240·87 240·76	240·82	5° 30′·0

Hence the simple deflections v_0, v_1 are obtained for the distances R_0, R_1 (without regard to signs)

$$v_0 = 2^\circ\ 45'\ 4''{\cdot}5, \text{ for } R_0 = 556{\cdot}75$$
$$v_1 = 5^\circ\ \ 5'\ 7''{\cdot}5, \text{ for } R_1 = 453{\cdot}25.$$

Consequently, if tang. v be developed according to the powers of R,

$$\text{tang. } v = 8305800\ R^{-3} - 4081300000\ R^{-5}$$

whence (see *Intensitas Vis Magneticæ*, Art. 21, 22),

$$\frac{M}{T} = 4152900.$$

With the comparatively great distance of the deflecting bar from the needle (equal to from 5 to 6 times the length of the needle), the determination of the coefficient of the second member of this equation (which is to be divided by the 5th power of the distance) is uncertain, and it is therefore better to disregard it. We then obtain for $\frac{M}{T}$ two values,

$$R_0^3 \text{ tang. } v_0 = 4146600$$
$$R_1^3 \text{ tang. } v_1 = 4143200,$$

viz. the mean of which may be taken, consequently,

$$\frac{M}{T} = 4144900,$$

which differs but little from the above value.

If to the results obtained we add lastly the time of vibration t, which was found to be

$$t = 6''\cdot 0586^*,$$

and if we assume $K = 49073500$, we obtain

$$MT = \frac{\pi^2 K}{t^2} = 13195000,$$

consequently

$$T = 1\cdot 7842.$$

We are not enabled to test and compare this result further, as a simultaneous measurement with the large magnetometer could not be executed at that time. When a new measurement of the earth's magnetic force is made in the Göttingen Observatory, the opportunity of comparison thus afforded will not be neglected.

The improvements, (represented in figs. 2, 6, 7, 8,) which, since the above was written, I have caused to be made in the transportable magnetometer, are designed to facilitate the use of the instrument in the open air, as in travelling it will be rare to meet with a suitable building free from iron for the execution of absolute measurements. It is not absolutely necessary that the whole of the observations for these purposes should be made in the open air; and on account of the liability to interruption from weather, it is desirable to reduce the number requiring this exposure as much as possible. In the improved construction I have given great care and consideration to this part of the subject, and have found it possible to arrange the observations in such manner that the greater part may be made in a room, including those which would be made to the greatest disadvantage in the open air.

Fig. 6. represents the tripod stand, on which the measuring apparatus, fig. 7, and the magnetometer, fig. 2, are to be placed and levelled, as shown in fig. 8. The measuring apparatus,

* The bar having been magnetized afresh for the experiments of vibration and deflection, had a shorter time of vibration than in the previous experiments on the moment of inertia.

fig. 7, required for the deflection experiments, consists of a copper-plate fitting on the tripod, and carrying the supporters of the deflecting-bar; each of these is formed of two converging tubes connected at their extremities, from whence proceeds a third tube provided with a graduation, and on this the deflecting-bar is to be placed: this tube forms also the reading telescope, and has the reading scale attached to it.

Fig. 8. represents the magnetometer placed on the measuring apparatus, which rests itself upon the tripod: the needle is suspended in a copper case, which acts as a damper in checking the vibrations. The mirror close below the needle is directed to the east. The whole of the eastern side of the copper case can be removed, to give access to the screw to which the suspension is fastened, and by which the inclination of the mirror may be corrected. In the middle of this side is an opening closed by a piece of plane glass, making a small angle with the vertical, in order that the reading telescope, which is directed to the mirror behind the glass, may not see a double image of the scale.

For the *measurement of the absolute intensity* the deflection experiments alone require to be made in the open air; the remainder may be made in a room if more convenient; for if the magnetism of the needle, which can be ascertained in a room, be known, the intensity of the earth's magnetism may be calculated from that of the needle, and from the experiments of deflection made in the open air*. It should be noticed, however, that the determination of the magnetism of the needle in such cases requires a complete measurement of the intensity to be gone through, including both the experiments of deflection and those of vibration, with and without the weights. The magnetism of the needle should also be determined either shortly before, or shortly after, the deflection experiments in the open air, because it is liable to alteration: and the temperature in the room and in the air should be as nearly the same as possible.

The experiments of deflection in the open air require only a

* The experiments of vibration might be made in the open air instead of those of deflection; but in such case the instrument would afford less certainty and less convenience.

solid foundation, on which the tripod may be placed and levelled; the measuring apparatus, resting on it, carries the deflecting bar, the telescope, and the scale, each in its due position relatively to the others; and the whole system can be turned upon the tripod without their displacement. The copper case of the magnetometer fits into the depression *a b*, fig. 8, by which its position is fixed relatively to all the other parts. The whole instrument is then turned on the tripod until the middle of the scale is seen in the reading telescope, and it is then ready for the deflection experiments.

The vernier of the deflecting bar being placed on the zero point of the graduation of the measuring apparatus, the deflection of the needle is observed. The deflecting bar is then reversed, and the observation repeated. The bar is then removed to the end of the measuring apparatus, and the vernier set to 1000^{mm} of the graduated scale, when the deflected position of the needle is again observed before and after the reversal of the bar. Let the four observed deflections be called m, m', n, n',—the absolute intensity of the magnetism of the needle, previously observed in a room, M,—and the arc-value of a division of the scale, determined also in a room (the torsion being taken into account), α,—then the absolute horizontal intensity of the earth's magnetism will be

$$I = \frac{2}{500^3} \cdot \frac{M}{\tan v},$$

where $v = \frac{1}{2}$ arc-tang. $\frac{1}{2}(m - m' + n - n')\,\alpha$.

This simple formula may be employed, because the small dimensions of the needle and bar, relatively to their distance apart, renders the next member (having the fifth power of the distance in the denominator) insensible.

Fig. 10 represents the theodolite used in observing the declination and its variations; it is provided with a verification telescope, having a small scale at the end: a larger scale is placed above the theodolite, perpendicular to the optical axis of the principal telescope.

The observation of the absolute declination may be divided into those parts which must be made in the open air, and those which may be made in a room. Fig. 11. represents in A the

cross-section of the tube of the magnetometer telescope, and in B C the scale; between A and B C is a transparent space; the theodolite must be so placed that the observer may look with the verification telescope through the space D towards the mirror of the magnetometer needle, and perceive the image of the scale attached to that telescope; he must first observe the position of the needle by this scale, and thence determine the angle ϕ (fig. 12.), which the optical axis of the verification telescope makes with the normal to the mirror of the magnetometer; he must then bisect objects of known azimuth with the principal telescope of the theodolite, and thence find the angle ψ corresponding on the divided limb to the direction of the principal telescope relatively to the north.

These are all the observations required to be made in the open air in determining the declination. The angle χ, Fig. 12, corresponding, on the graduated limb, to the parallel position of the optical axes of the two telescopes of the theodolite, can be ascertained in a room; as can also the angle ϱ which the magnetic meridian makes with the normal of the mirror belonging to the needle. Hence we obtain

$(\chi-\psi)$ the angle which the optical axis of the verification telescope makes with the true meridian.

$(\chi-\psi)-\phi$, the angle which the mirror-normal of the needle makes with the true meridian.

$\varrho-\{(\chi-\psi)-\phi\}$ the angle which the magnetic meridian makes with the true meridian.

The angle χ is found by placing a plane mirror before the verification telescope, and viewing in the telescope the reflected image of a vertical thread suspended over the middle of the object glass; a vertical thread is also suspended over the middle of the principal telescope, and the telescope adjusted to its reflected image; the reading on the circle gives the angle χ, supposing the collimation error of the principal telescope to remain unaltered when the eye-piece is adjusted to distant objects; otherwise the alteration must be sought by reversing the telescope, and applied as a correction to the reading on the circle.

The angle ϱ is determined by directing the principal telescope of the theodolite from B (fig. 12.) to C, a second needle suspended in the wooden case, as represented in fig. 3; the

verification telescope is directed on the first needle A, in the copper case as in the open air. The needle C is furnished either with a collimator or a mirror, and is capable of reversal. The direction of its magnetic axis is next to be found, *i. e.* the angle μ, to which the theodolite must be adjusted, in order that the optical axis of its principal telescope may be parallel with the direction of the magnetic axis of the needle C, whence the angle ϱ $(= \pi - (\chi - \phi) + \mu)$ is obtained, if the two needles A and C are sufficiently distant apart to exert no sensible influence on each other, so that their magnetic axes may be regarded as parallel. But if this be not the case, it is easy to determine the angle ν formed by the magnetic axes of the two needles*, and to add it as a correction to the value, as above, of ϱ; *i. e.*

$$\varrho = \pi - (\chi - \phi) + \mu + \nu.$$

The suspension of the needle in the wooden case is so contrived, that it may be used either as an unifilar or as a bifilar magnetometer. This contrivance is represented in fig. 9. The variations of the declination and of the horizontal intensity can thus be observed at the same time; the former with the magnetometer in the copper case, and the latter with the magnetometer in the wooden case. In preparing for the latter observations, the telescope of the theodolite is to be directed perpendicularly to the magnetic meridian, and the magnetometer in the wooden case is to be placed in the same direction. The time of vibration t of the needle, with the unifilar suspension, must be determined, if not already known, which it will generally be, from the experiments of vibration belonging to the measurement of the absolute intensity. The unifilar suspension must then be changed for the bifilar without altering the direction of the magnetic axis, and the time of vibration must be observed afresh, the distance apart of the suspension threads being in-

* From the propositions contained in the *Resultate* for 1837, page 22 *et seq.*, it follows that if $ABC = 90°$, $ACB = \alpha$, and $AC = \nu$, and if m and m' denote respectively the magnetism of the needles A and C,

$$\nu = \frac{3}{2} \sin 2\alpha \, . \, \frac{m - m'}{r^3 \mathrm{T}}.$$

The value of m and m' must be determined by the deflections δ and δ' of a compass needle placed successively east and west at the distance d, namely

$$\frac{m}{\mathrm{T}} = \frac{\delta d^3}{2}, \quad \frac{m'}{\mathrm{T}} = \frac{\delta' d^3}{2}.$$

creased until t' is about $= 0{\cdot}6871\ t$. The torsion circle must then be turned until the middle of the scale appears in the field of view of the telescope, and the time of vibration t'' observed. The magnetometer is then in the transversal position proper for observing the variations of intensity, and the value of the scale divisions may easily be calculated from the observed times of vibration t, t', t''; namely, if σ denote the arc-value of a division of the scale in parts of radius, the value of a division of the scale, in parts of the whole horizontal intensity, is

$$\frac{t^2}{t''^2} \cdot \sigma = \sqrt{\left(1 - 2\frac{t'^2}{t^2}\right)} \cdot \frac{t^2}{t'^2} \cdot \sigma.$$

Explanation of the Figures, Plate XXV.

Fig. 1. *a, b, c, d* is a vertical longitudinal section of the copper case of the magnetometer, with the needle *e, f* suspended by a silk thread *g, h*. The needle is seen to be pierced through its length, and provided at the extremity *f* with a lens; it is inclosed in a copper tube *k, l, m, n*, and can be turned by means of a key *o, p*, which is accessible by an opening in the copper case. In doing this the copper tube is held by two screws *q, r*, and two projections *s, t*. The mirror *u, v* is seen above the copper case, near the axis of rotation of the needle. A dotted line indicates how the telescope of the theodolite, fig. 10, is directed, both to the needle and to the lens at its end *f*, and also to the mirror *u, v*. It is also seen how the inclination of the mirror may be regulated by the screw *w*, that the image of the scale placed above the telescope at *a*, fig. 10, may appear in the field of view. This fig. is half the size of the instrument itself.

Fig. 2. represents a magnetometer, which differs from the one just described in not being adapted for *complete* measurements of the declination. The collimator is omitted, and the needle cannot be reversed. The mirror *a, b, c, d* is inclosed in the copper case, and is parallel to the plane of the magnetic meridian; the inclination of the mirror is regulated by the screw at *e*; the copper case forms an unbroken damper round the needle, except at the aperture for the suspension thread; the mirror is observed through a glass plate in one of the sides of the copper case. This figure is also half size.

Fig. 3. *a, b, c, d, e* represents the wooden case, in which either of the instruments shown in figs. 1. and 2. are packed for travelling. The lid, with the tube *a, b, c* which is fastened to it, is taken off, the instrument

placed inside, and the box closed again. When observations are made, this box serves for suspending a second needle, the time of vibration of which is required for the measurement of the absolute intensity; this second needle *f*, *g* is provided at both ends with mirrors, one of which serves for observing the scale. The needle rests on two supports *h*, *k*, attached to a small measuring bar *m*, *n*, over which passes a thread carrying the weights *p*, *q*, which serve to increase the moment of inertia of the oscillating needle. The needle can be turned in the supports *h*, *k*, and may be reversed; rendering it available, in absolute measurements of the declination, as an auxiliary needle, when the instrument represented in fig. 2. is used, the needle of which is not reversible. For this purpose, instead of a needle with a mirror, one with a collimator, fig. 13, may be placed in *h*, *k*. It consists of a magnetic steel tube *a*, *b*, *c*, *d*, carrying at the end *a*, *c*, an achromatic object-glass; and at its other extremity a sliding tube of brass *e*, *f*, *g*, *h*, provided with a glass micrometer in the focus of the object-glass. It will be seen also by fig. 3. that this needle is suspended to two threads, the upper points of attachment of which are *r* and *s*. The threads are conducted over a roller *z* to give them equal tension, and are united in one from *u* to *v*, forming an unifilar suspension, which may be converted into a bifilar by opening out the apparatus α, β, γ, δ, which is done by pressing down the knob *w* by the screw *t*, and disengaging the threads from the pins *x*, *y*, as represented in fig. 9. Fig. 3. is also half size.

Fig. 4. A, is the theodolite carrying two telescopes and two scales; one telescope and one scale serve for observing the unifilar magnetometer B, and the other telescope and scale for observing the bifilar magnetometer C. The figure gives the angles which the instruments ought to form with each other.

Fig. 5. is a graphical representation of the variations of the declination observed on the 13th of April, 1839, at Göttingen, simultaneously in the magnetic observatory, and with the transportable magnetometer.

Fig. 6. is the tripod on which the magnetometer, fig. 2, is placed and levelled.

Fig. 7. is the apparatus required for the experiments of deflection. *a*, *b*, *c*, *d* is a copper disc which fits on to the tripod, fig. 6; *e*, *f*, *g*, *h*, and *k*, *l*, *m*, *n*, are arms screwed to the copper disc at *e*, *f* and *k*, *l*; one arm carries the telescope *p*, *q*, to which the scale *r*, *s* is attached, and upon which the deflecting bar *u*, *v* is to be laid; the other arm carries a tube on which the deflecting bar is also laid, but which could not be conveniently represented in the figure. Between *e*, *f* and *k*, *l* the magnetometer (fig. 2.) is placed.

Fig. 8. is a smaller side-view of the magnetometer represented in fig. 2, in its proper relative position to the measuring apparatus, fig. 7, and resting on the tripod, fig. 6. In this view the needle is seen only by its circular cross-section, and the glass plate is shown, in the side of the case which permits the image of the scale, reflected from the mirror, to be observed with the telescope.

Fig. 9. is explained in the description of fig. 3.

Fig. 10. represents the theodolite with the verification telescope: two scales are seen, one of which, *a*, is applied in such manner that its middle corresponds to the prolongation of the vertical axis of rotation of the theodolite; the other, *b*, *c*, is attached in front of the object-glass of the verification telescope. It is very narrow, in order to intercept the less light.

Figs. 11, 12 and 13. are explained in the text.

W. Weber.

Article XIX.

An extract from Remarks on the Term-Observations for 1839, *of the German Magnetic Association. By* Wilhelm Weber.

(With a Plate.)

[From the *Resultate aus den Beobachtungen des magnetischen Vereins im Jahre* 1839.]

In concluding this notice, I wish to call attention to the observations made in the high northern latitudes in 1838 and 1839, for which we are indebted to the zeal and perseverance of the French *savans*, MM. Lottin, Bravais and Martin, and of the Swedish naval officers, Lieutenants Siljestrom and Siljehook, who joined the French expedition to Spitzbergen and Finmarken: we may derive instruction from these observations in regard to the arrangement of future researches of the same nature in those regions. In looking at the Plate, XXVI., it is obvious at the first glance, that the beautiful accordance in the variations of the magnetic elements, which had been hitherto invariably observed, from Catania, Rome, Milan, &c., to Upsala in the north, ceases when we proceed still further north; so that in comparing the curves of Upsala and Alten (in Finmarken, lat. 69° 58′) they would scarcely be recognized as belonging to the same term. There is no doubt as to the correctness of the observations, as the voyagers undertook the additional trouble of occasionally observing Gambey's needle simultaneously with the magnetometer, and the movements of both were found to be in accord. If therefore these observations sufficiently assure us of the great difference between the magnetic changes in those more northern districts and in Upsala, the important conclusion follows, that future term-observations in these very high latitudes will only be rendered truly valuable by the establishment of intervening stations, which may show the gradual passage of the one system of changes into the other; or by having a group of several stations around Alten and its vicinity, which will afford a sufficient interest by their mutual comparison independently of others, as it is to be expected that great differences should there manifest themselves at small distances. Such observations would be available for inquiries, for which those

made elsewhere are little or not at all adapted; in particular we might, by their means, determine *most securely* whether the forces which cause the variations have their seat *above* or *below* the surface of the earth. Without this multiplication of stations in its vicinity, observations of the variations at Alten will have a much inferior value, as they differ so greatly from those at the nearest present station, Upsala, of which we may convince ourselves by inspection of the curves of declination and horizontal intensity on the 23rd of February, 1839, shown in Plate XXVI. The three declination curves represent the variations of that element from noon to 10 P.M., Göttingen mean time, at Alten, Upsala, and Göttingen, and are all on the same scale. The two curves of the horizontal intensity are for the same period, and represent the changes at Alten and at Göttingen, which was the next most northern station at which the intensity was observed during that term. We cannot perceive in the two latter curves even that trace of resemblance which is visible in those of the declination.

W. Weber.

Article XX.

Results of the Daily Observations of Magnetic Declination during six years at Göttingen. By Dr. B. Goldschmidt.

[From the *Resultate aus den Beobachtungen des magnetischen Vereins im Jahre* 1839.]

In the volume of the *Resultate* for 1836, M. Gauss communicated the results of the observations of the magnetic declination, made daily in the magnetic observatory at Göttingen, from the 17th March, 1834, to the 31st of March, 1837, and combined them in various ways for the purpose of determining the march of the declination*. Since that period these observations have been continued uninterruptedly by me according to the same plan, and we have now before us the determinations of six years, which I propose to consider in this treatise.

To the mean values of the declination for the several months of the three first years, published as above, we have now to add the following:—

Month.	1837 to 1838.		1838 to 1839.		1839 to 1840.	
	8 A.M.	1 P.M.	8 A.M.	1 P.M.	8 A.M.	1 P.M.
	′ ″	′ ″	′ ″	′ ″	′ ″	′ ″
April	21 52·1	40 42·2	18 08·9	35 56·7	14 43·8	28 43·5
May	23 17·3	38 35·2	18 43·9	35 46·1	15 16·7	28 15·0
June	22 46·2	38 24·8	17 40·7	35 06·2	13 54·1	27 15·5
July	21 33·3	36 55·4	18 47·6	33 48·2	14 27·6	28 16·6
August	24 22·2	37 51·9	18 43·9	34 59·4	13 40·9	30 07·0
September ...	25 02·5	37 19·1	18 17·1	33 17·5	13 41·8	27 26·5
October	25 50·0	37 00·2	19 58·7	30 48·3	14 47·4	25 53·0
November ...	25 47·5	33 12·7	22 06·6	28 14·4	16 01·8	23 08·9
December ...	25 51·4	31 14·5	21 34·3	26 19·0	16 54·5	21 02·6
January	25 25·3	33 36·2	21 01·6	27 35·1	15 41·5	20 48·6
February ...	23 55·3	33 37·8	20 01·0	27 29·8	13 53·1	22 15·9
March	20 46·4	35 29·6	18 09·6	29 52·4	11 14·4	23 42·4
The number of degrees is throughout 18.						

We will now proceed to combine these numbers in the same manner as was done with the observations of the first three years, beginning with the deduction of the differences between the forenoon and afternoon declinations. These differences, the

* Translated in the Scientific Memoirs, vol. ii. Part V. pp. 51 to 65.

monthly mean values of which have all the same sign, are exhibited in the following table:—

	1837 to 1838.	1838 to 1839.	1839 to 1840.
	′ ″	′ ″	′ ″
April	18 50·1	17 47·8	13 59·7
May..................	15 17·9	17 02·2	12 58·3
June	15 38·6	17 25·5	13 21·4
July..................	15 22·1	15 00·6	13 49·0
August	13 29·7	16 15·5	16 26·1
September	12 17·6	15 00·4	13 44·7
October	11 10·2	10 49·6	11 05·6
November	7 25·2	6 07·8	7 07·1
December	5 23·1	4 44·7	4 08·1
January	8 11·0	6 33·5	5 07·1
February............	9 42·5	7 28·8	8 22·8
March	14 43·2	11 42·8	12 28·0
Mean......	12 17·6	12 09·9	11 03·2

The following table contains the mean values for the different months resulting from the observations in these three years, and from the observations in the whole six years from 1834 to 1840; also, for more convenient comparison, the mean values deduced in the first volume of the *Resultate*, for the years 1834 to 1837, are here repeated:

	1834 to 1837.	1837 to 1840.	1834 to 1840.
	′ ″	′ ″	′ ″
April	13 53·5	16 52·5	15 23·0
May..................	13 29·1	15 06·1	14 17·6
June..................	12 27·0	15 28·5	13 57·8
July..................	12 09·4	14 43·9	13 26·6
August	13 03·3	15 23·8	14 13·5
September	11 48·4	13 40·9	12 44·7
October	10 03·3	11 01·8	10 52·5
November	6 51·1	6 53·4	6 52·2
December	5 01·4	4 45·3	4 53·4
January	6 42·0	6 37·2	6 39·6
February............	7 22·4	8 31·4	7 56·9
March	11 54·2	12 58·0	12 26·1
Mean......	10 23·8	11 50·2	11 07·0

The progression of these differences in the period 1837–1840 is quite analogous to that deduced from the first period, the least value being in both in December, and the greatest in April. The variations, however, in the mean values in different years,—the discrepancies in the values for the same months in the different years,—and the difference of nearly two minutes between numbers, each of which is derived from the observations of three

years,—manifest that even six years' observations are not sufficient to give with certainty the mean value of the difference between the declinations at 8 A.M. and 1 P.M., although they leave no doubt as to the general progression.

The following periodical function, which represents the mean of each month, derived from the six years' observations, is to be regarded, therefore, only as an attempt to reproduce this progression by a formula; longer continued observations may still perhaps occasion considerable alteration in the coefficients. In this formula ϕ denotes the number of months from the middle of April multiplied by 30°.

$$11' \, 7''\cdot0 + 124''\cdot0 \cos \phi + 239''\cdot4 \sin \phi + 84''\cdot8 \cos 2\,\phi - 63''\cdot6 \sin 2\,\phi$$
$$+ \, 20''\cdot4 \cos 3\,\phi + 9''\cdot5 \sin 3\,\phi + 23''\cdot5 \cos 4\,\phi - 6''\cdot0 \sin 4\,\phi$$
$$+ \, 0''\cdot8 \cos 5\,\phi - 26''\cdot4 \sin 5\,\phi + 2''\cdot5 \cos 6\,\phi.$$

In the second period of three years, eight exceptional instances have occurred of days on which the declination was greater at 8 A.M. than at 1 P.M.; of these seven were in winter months, and only one in the summer months, being almost the same proportion as in the former three years. The following table contains the days, and the amount, by which the forenoon declination exceeded that of the afternoon.

	′ ″		′ ″
1837. October 23	3 55·3	1839. February 14	1 06·0
December 13	1 22·8	May 6	1 43·3
December 15	2 07·7	October 23	0 59·8
1838. January 5.........	5 57·2	1840. January 4.........	2 40·1

It is worthy of notice, that of the twenty-two exceptional cases of this nature which have occurred since the commencement of the observations in 1834, only two (December 15th, 1837, and February 14th, 1839) have been occasioned by a derangement of the declination in the afternoon; there are two others, in which the anomaly was produced by derangements both in the forenoon and afternoon; whilst the remaining eighteen originate in considerable irregularities in the forenoon declination. We may hence conjecture that great anomalies are of more frequent occurrence in the forenoon than in the afternoon, which appears to be confirmed by the fluctuations on successive days, which will be discussed in the sequel.

In comparing the mean declination of each month with that

of the same month in the succeeding year, we obtain the secular variation, which has the same signs in the whole thirty-six results.

	Third Year.		Fourth Year.		Fifth Year.	
	8 A.M.	1 P.M.	8 A.M.	1 P.M.	8 A.M.	1 P.M.
	′ ″	′ ″	′ ″	′ ″	′ ″	′ ″
April	4 41·8	3 00·4	3 42·2	4 45·5	3 25·1	7 13·2
May	4 43·5	6 02·0	4 33·4	2 49·1	3 27·2	7 31·1
June	4 48·9	4 27·6	5 05·5	3 18·6	3 46·6	7 50·7
July	5 20·9	5 30·6	2 45·7	3 07·2	4 20·0	5 31·6
August	1 20·2	3 53·1	5 38·3	2 52·2	5 03·0	4 52·4
September	1 12·1	3 40·5	6 45·4	4 01·6	4 35·3	5 51·0
October	1 44·0	3 32·6	5 51·3	6 11·9	5 11·3	4 55·3
November	3 33·5	3 41·6	3 40·9	4 58·3	6 04·8	5 05·5
December	3 22·3	4 32·3	4 17·1	4 55·5	4 39·8	5 16·4
January	2 10·0	4 09·9	4 23·7	6 01·2	5 20·1	6 46·5
February	3 40·3	2 50·5	3 54·3	6 08·0	6 07·9	5 13·9
March	4 57·8	3 34·6	2 36·8	5 37·2	6 55·2	6 10·0
Mean	3 25·2	4 04·6	4 26·2	4 33·9	4 54·7	6 01·5

We find in these values, that the secular decrease, derived from the afternoon observations, is greater for all the three years than that which results from the forenoon observations; this circumstance is connected with the decrease of the difference between the forenoon and afternoon observations in these three years; in the two first years the contrary was the case.

If we now combine the yearly mean values of the secular change derived from both forenoon and afternoon observations, we obtain the following values of the mean decrease:—

Year 1.	Year 2.	Year 3.	Year 4.	Year 5.
2′ 36″·5	4′ 55″·9	3′ 44″·9	4′ 30″·0	5′ 28″·1

We will investigate subsequently the law of the secular decrease of the declination, as far as our observations enable us to do.

The following table shows the mean values of the secular variation for the several months:—

	Year I. and II.	Year III. to V.	Year I. to V.
	′ ″	′ ″	′ ″
April	3 13·8	4 18·0	3 58·5
May..................	2 46·4	4 51·0	4 01·2
June	3 48·1	4 51·3	4 26·0
July..................	4 14·1	4 26·0	4 21·2
August	5 07·9	3 56·5	4 25·1
September	5 04·1	4 21·0	4 14·2
October	3 29·6	4 34·4	4 08·5
November	3 36·8	4 30·8	4 09·
December	3 36·7	4 30·6	4 09·0
January	3 41·1	4 48·6	4 21·6
February............	3 52·2	4 39·2	4 20·4
March	3 43·6	4 58·6	4 28·6
Mean......	3 46·2	4 34·0	4 15·4

So much regularity appears in the numbers in the last column, that we may hope that the mean value of the secular change 4′ 15″·4, corresponding to the 1st of April, 1837, may not be far from the truth.

We now proceed to consider the mean values of the declination for each twelve months of our six years' observations, as in the following table:—

	8 A.M.	1 P.M.	Mean.
1834 to 1835 ...	18° 37′ 12″·5	18° 45′ 27″·0	18° 41′ 19″·75
1835 to 1836 ...	33 42·0	43 44·8	38 43·4
1836 to 1837 ...	27 20·3	40 14·6	33 47·45
1837 to 1338 ...	23 52·5	36 10·0	30 01·25
1838 to 1839 ...	19 26·2	31 36·1	25 31·15
1839 to 1840 ...	14 31·5	25 34·6	20 03·05
Mean......	18 26 00·8	18 37 07·8	18 31 34·3

The mean values of the several years correspond to the middle day of the period comprised; *e. g.* October 1st, 1834, &c. The means of the six years give the mean declination for the 1st of April, 1837.

Under the supposition that the mean decrease of the declination is proportional to the time, I have calculated by the method of least squares, from the numbers contained in the last column, the following formula for the declination δ:—

$$\delta = 18° \, 42' \, 16''\cdot231 - 4' \, 16''\cdot756 \, . \, t,$$

where t denotes the interval elapsed since October 1st, 1834, expressed in years. The values of the mean declination com-

puted by this formula, and their deviations from the observed values, are as follows:—

	Computed Declination.	Difference.		Computed Declination.	Difference.
1834 to 1835...	18° 42′ 16″·231	+ 56″·481	1837 to 1838...	18° 29′ 25″·963	− 35″·287
1835 to 1836...	37 59·475	− 43·925	1838 to 1839...	25 9·207	− 21·943
1836 to 1837...	33 42·719	− 4·731	1839 to 1840...	20 52·451	+ 49·401

According to this table the mean deviation of a determination of the declination deduced from one year is 48″·942; the mean error to be feared in the determination of the absolute part of the formula is 34″·92; and the mean error to be feared in the determination of the coefficient of t is 11″·53.

It is more natural to suppose that the decrease is uniformly accelerating than constant; therefore the declination may be represented by the formula $a + b\,t + c\,t^2$. Giving t the same signification as before, we obtain by the combination of the six data, using the method of least squares,

$$\delta = 18^\circ\ 41'\ 31''\!\cdot\!442 - 3'\ 09''\!\cdot\!514\ t - 0'\ 13''\!\cdot\!453\ t^2;$$

and the values of δ, computed by this formula, as well as the deviations from the observed values, are as follows:—

	Computed Declination.	Difference.		Computed Declination.	Difference.
1834 to 1835...	18° 41′ 31″·442	+ 11″·672	1837 to 1838...	18° 30′ 01″·830	+ 0″·580
1835 to 1836...	38 08·473	− 34·927	1838 to 1839...	25 18·152	− 12·998
1836 to 1837...	34 18·604	+ 31·154	1839 to 1840...	20 07·570	+ 4·520

The sum of the squares of the remaining deviations is 2515·4; hence the mean deviation of a single year's determination, so far as it can be derived from six years' observations, is 28″·96. The weights of a, b, c are 1·317, 1·376, and 37·34, where the weight of a mean value of the declination, deduced from a whole year, is unity; with 28″·96 as the mean deviation of such a mean value, we have the mean errors of a, b and c, 25″·23, 24″·68, and 4″·74. Our formula gives 18° 52′ 38″ for the maximum of the declination, and the corresponding $t = -\,7{\cdot}043$; so that on the 14th of September, 1827, the declination had become retrograde. It need scarcely be remarked, that both these numbers are uncertain, as the coefficient of t^2, on which the determination of the time of the maximum principally depends, is uncertain to one-third of its whole value. Unfortunately we have

no direct determination of the year in which the declination began to decrease in Göttingen.

We obtain from the formula the yearly change of the declination $- 3' \ 22''{\cdot}967 - 26''{\cdot}906 \ t$, corresponding to the interval $1834 + t$ to $1835 + t$, where t denotes the time elapsed since the 1st of October, 1834, expressed in parts of a year.

The influence of the season of the year on the mean values of the declination in the several months, has already been noticed in discussing the differences between the forenoon and afternoon declinations; by comparing the monthly means with the declination deduced from the whole year, we may perceive how great this influence is, and the nature of the effects it produces. This comparison gives the following differences for the three years 1837 to 1840:—

Declination, 8 A.M.

	Fourth Year.	Fifth Year.	Sixth Year.	Mean.
	′ ″	′ ″	′ ″	′ ″
April	− 2 00·4	− 1 17·3	+ 0 12·3	− 1 1·8
May	− 0 35·2	− 0 42·3	+ 0 45·2	− 0 10·8
June	− 1 06·3	− 1 45·5	− 0 37·4	− 1 09·7
July	− 2 19·2	− 0 38·6	− 0 03·9	− 1 00·6
August	+ 0 29·7	− 0 42·3	− 0 50·6	− 0 21·1
September	+ 1 10·0	− 1 09·1	− 0 49·7	− 0 16·3
October	+ 1 57·5	+ 0 32·5	+ 0 15·9	+ 0 55·3
November	+ 1 55·0	+ 2 40·4	+ 1 30·3	+ 2 01·9
December	+ 1 58·9	+ 2 08·1	+ 2 23·0	+ 2 10·0
January	+ 1 32·8	+ 1 35·4	+ 1 10·0	+ 1 26·1
February	+ 0 02·8	+ 0 35·8	− 0 38·4	+ 0 00·1
March	− 3 06·1	− 1 16·6	− 3 17·1	− 2 33·3

Declination, 1 P.M.

	Fourth Year.	Fifth Year.	Sixth Year.	Mean.
	′ ″	′ ″	′ ″	′ ″
April	+ 4 32·2	+ 4 20·6	+ 3 08·9	+ 4 00·6
May	+ 2 25·2	+ 4 10·0	+ 2 40·4	+ 3 05·2
June	+ 2 14·8	+ 3 30·1	+ 1 40·9	+ 2 28·6
July	+ 0 45·4	+ 2 12·1	+ 2 42·0	+ 1 53·2
August	+ 1 41·9	+ 3 23·3	+ 4 32·4	+ 3 12·5
September	+ 1 09·1	+ 1 41·4	+ 1 51·9	+ 1 34·1
October	+ 0 50·2	− 0 47·8	+ 0 18·4	+ 0 06·9
November	− 2 57·2	− 3 21·7	− 2 25·7	− 2 54·9
December	− 4 55·5	− 5 17·1	− 4 32·0	− 4 54·9
January	− 2 33·8	− 4 01·0	− 4 46·0	− 3 46·9
February	− 2 32·2	− 4 06·3	− 3 18·7	− 3 19·1
March	− 0 40·4	− 1 43·7	− 1 52·4	− 1 25·5

The numbers in the last column are still charged with the secular change, from which we may free them by reducing

them all to the 1st of October, employing $4' \ 57''{\cdot}1$ as the secular change in the year 1838 to 1839. We thus obtain

	8 A.M.	1 P.M.	Mean.
	′ ″	′ ″	″
April	− 3 18·0	+ 1 44·4	− 46·8
May..................	− 2 02·2	+ 1 13·8	− 24·2
June	− 2 36·4	+ 1 01·9	− 47·2
July..................	− 2 02·5	+ 0 51·3	− 35·6
August	− 0 58·2	+ 2 35·4	+ 48·6
September	− 0 28·7	+ 1 21·8	+ 26·6
October	+ 1 07·7	+ 0 19·3	+ 43·5
November	+ 2 39·0	− 2 17·8	+ 10·6
December	+ 3 11·9	− 3 53·0	− 20·5
January	+ 2 52·8	− 2 20·2	+ 16·3
February............	+ 1 51·4	− 1 27·7	+ 11·9
March	− 0 17·1	+ 0 50·7	+ 16·8

Lastly, we obtain from the six years the following mean values of these differences :—

	8 A.M.	1 P.M.	Mean.
	′ ″	′ ″	″
April	− 2 56·8	+ 1 19·3	− 48·7
May..................	− 1 58·8	+ 1 11·9	− 23·4
June	− 1 51·5	+ 0 59·3	− 26·1
July..................	− 1 17·2	+ 1 02·4	− 7·4
August	− 0 38·5	+ 2 28·0	+ 54·7
September	− 0 35·8	+ 1 01·7	+ 13·0
October	+ 0 38·5	+ 0 04·1	+ 21·3
November	+ 2 23·5	− 1 51·2	+ 16·1
December	+ 2 42·6	− 3 21·0	− 24·2
January	+ 2 21·5	− 2 05·9	+ 7·8
February............	+ 1 36·4	− 1 33·9	+ 1·3
March	− 0 24·0	+ 0 55·1	+ 15·6

The numbers of the first column give the differences between the forenoon declination in the several months, and the mean forenoon declination in the whole year; applied with their sign to the mean declination of the year, they give the mean forenoon declinations of the several months freed from the secular change, so far as the latter can be derived from six years' observations. The same remark applies to the second column in respect to the afternoon declinations.

If we represent these two columns by periodical functions, we find for the first

$$- 83''{\cdot}7 \cos \phi - 118''{\cdot}3 \sin \phi - 45''{\cdot}8 \cos 2 \phi + 11''{\cdot}2 \sin 2 \phi$$
$$- 12''{\cdot}7 \cos 3 \phi - 9''{\cdot}2 \sin 3 \phi - 18''{\cdot}5 \cos 4 \phi + 13''{\cdot}2 \sin 4 \phi$$
$$- 11''{\cdot}3 \cos 5 \phi - 0''{\cdot}3 \sin 5 \phi - 4''{\cdot}9 \cos 6 \phi.$$

For the second column we find

$+ 40''\cdot4 \cos\phi + 121''\cdot1 \sin\phi + 39''\cdot1 \cos 2\phi - 52''\cdot2 \sin 2\phi$
$+ 7''\cdot8 \cos 3\phi + 0''\cdot2 \sin 3\phi + 5''\cdot0 \cos 4\phi + 7''\cdot2 \sin 4\phi$
$- 10''\cdot6 \cos 5\phi - 26''\cdot7 \sin 5\phi - 2''\cdot4 \cos 6\phi,$

where ϕ denotes the number of months elapsed since the middle of April multiplied by 30°.

In *eleven* months we perceive a confirmation of the remarkable result previously deduced from the consideration of the observations of the first three years, namely, that the forenoon and afternoon declinations deviate from their mean values in opposite directions. October is the only exception; and, viewing the small amount of the differences in that month, and the degree of uncertainty which still remains, this exception may perhaps disappear when the observations shall have been longer continued. In the four winter months, November to February, the forenoon declination is greater, and the afternoon declination less, than their respective mean values; and both these circumstances contribute, during this portion of the year, to bring the whole difference below its mean value. From March to September the opposite effect takes place. These opposite deviations, moreover, being, on an average, nearly of equal magnitude, nearly counterbalance each other in their means, which are represented in the last column. The mean being also very small in the exceptional month of October, the law enounced in the *Resultate* for 1836*, appears to be confirmed, namely, that the mean result of the declinations observed at 8 A.M. and 1 P.M. does not contain, apart from the irregular anomalies and the secular decrease, any important fluctuations dependent on season.

Lastly, we have to consider the fluctuations of the declination from one day to another. In the *Resultate* for 1836† the following definition was given of the term "*fluctuation,*" namely, "the difference from the declination of the preceding day at the same hour;" and, by analogy with what are called *mean errors* of observation, the *mean fluctuation*, during any given interval of time, is the square root of the mean of the squares of the several fluctuations. It was further remarked, that when several equal intervals, or intervals considered as equal, are united in one, the arithmetical mean of the partial mean fluctuations

* Scientific Memoirs, vol. ii. page 62. (Part V.) † *Ibid, loc. cit.*

must not be taken as the general mean; but we must revert to the squares, and take the square root of their arithmetical mean. The results of the last three years, calculated in this manner, and expressed in seconds, are contained in the following table.

Mean Fluctuation of the Declination during the three years from 1837 to 1840.

	8 A.M.			1 P.M.		
	Fourth Year.	Fifth Year.	Sixth Year.	Fourth Year.	Fifth Year.	Sixth Year.
April	316	149	162	199	229	152
May............	319	157	266	211	193	176
June	262	208	205	211	236	159
July............	189	224	214	332	158	183
August	234	119	194	139	209	216
September ...	232	240	267	215	167	246
October	286	272	267	278	210	205
November ...	145	147	98	257	189	143
December ...	174	84	108	250	129	132
January	302	179	220	208	254	154
February......	274	133	97	241	217	195
March	195	271	118	184	145	174
Mean......	252	192	193	232	198	179

The following table contains the mean values of the fluctuations for the several months of the intervals 1834 to 1837, 1837 to 1840, and of the whole interval 1834 to 1840.

	8 A.M.			1 P.M.		
	I. to III.	IV. to VI.	I. to VI.	I. to III.	IV. to VI.	I. to VI.
April	147	223	189	180	196	188
May............	207	260	235	185	194	190
June	181	227	205	162	201	183
July............	250	208	230	193	241	218
August	262	188	228	225	191	209
September	241	246	244	159	210	186
October	222	274	249	210	232	221
November	218	131	180	158	200	180
December	206	127	171	182	179	180
January	196	238	218	181	208	195
February......	143	183	164	165	217	193
March	228	203	216	183	168	176
Mean......	211	214	213	183	203	193

In the means deduced from the six years, there are nine cases in which the forenoon fluctuation is greater than that of the afternoon; in one instance they are equal; and in two the

afternoon preponderates. The mean of all the forenoon fluctuations gives 3′ 33″, that of the afternoon 3′ 13″. The greatest fluctuation in the last three years, in the forenoon, was on the 5th of January, 1838, when the declination was 16′ 51″ greater than the preceding day; and in the afternoon, on the 12th of July, 1837, when the declination was 15′ 57″ greater than on the 11th. This last is the greatest afternoon fluctuation that has occurred during the six years; the greatest forenoon fluctuation was the one already mentioned in the *Resultate* for 1836, occurring between the 8th and 9th of October, 1835, and amounting to 20′ 01″.

From the combination of the forenoon and afternoon fluctuations we obtain the following mean values:—

	IV.	V.	VI.	I. to III.	IV. to VI.	I. to VI.
April	264	193	157	164	210	189
May	270	176	226	196	227	213
June	238	222	183	172	216	194
July	270	194	199	223	224	224
August	193	171	205	244	190	219
September	224	207	257	204	230	217
October	282	243	238	216	255	236
November	209	169	123	191	170	180
December	215	109	121	195	156	176
January	259	220	190	189	217	207
February	258	180	154	155	202	179
March	190	218	149	206	187	197
Mean values.						
July } December }	234	187	197	213	207	210
Remaining } months }	248	202	178	181	212	197
The whole year	242	195	187	198	209	204

From the numbers in the last column we cannot yet recognize any influence of season on the fluctuations; longer continued observations will be required to enable us to arrive at a conclusion on this point; they may also alter materially the mean value (3′ 24″) of the fluctuations, and still more the magnitude of the difference between the forenoon and afternoon fluctuations, supposing the fact of a difference to be confirmed.

The results drawn from the six years' observations rest on 4323 single observations, of which 2164 were made in the forenoon, and 2159 in the afternoon. Of the sixty-one observations which are wanting to correspond with the number of days in the six years, thirty-two were necessarily omitted on account of

alterations required in the apparatus or in the building, and other accidental hindrances. The remaining twenty-nine observations were made, but were subsequently rejected as uncertain, on discovering that the free motion of the bar had been impeded by spider threads. Accidents of this nature were noticed in the *Resultate* for 1836. When a peculiarity in the reading leads to a suspicion of such an occurrence, it is easily tested by putting the bar in vibration, when the rapid decrease of the arc, and the unusually short time during which the bar continues to oscillate, afford indications of the existence of a disturbing cause. Should the thread be broken, on the bar being set in motion, its previous influence may still be inferred, should the direction of the bar, as concluded from the elongations, differ much from its position before it was put in motion. It is desirable that the freedom of the bar should be frequently proved in this manner.

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Fig: 4.

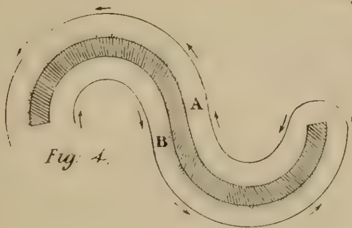


Fig 6.

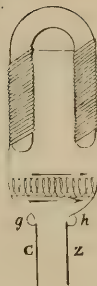


Fig. 5.

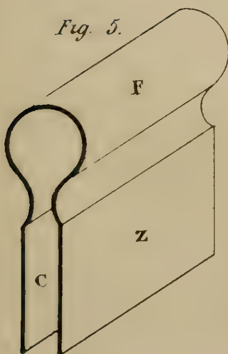
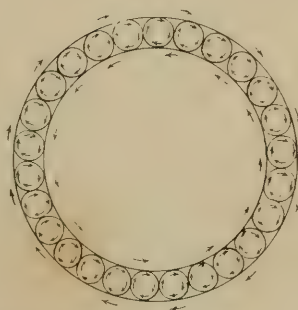


Fig. 1.



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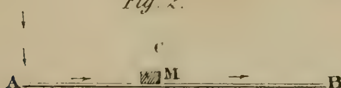

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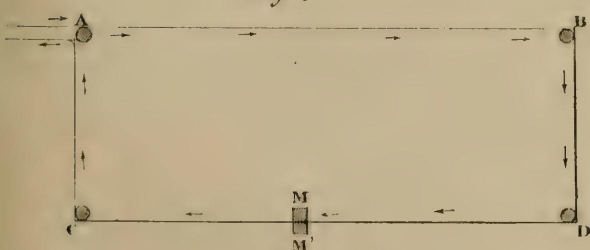

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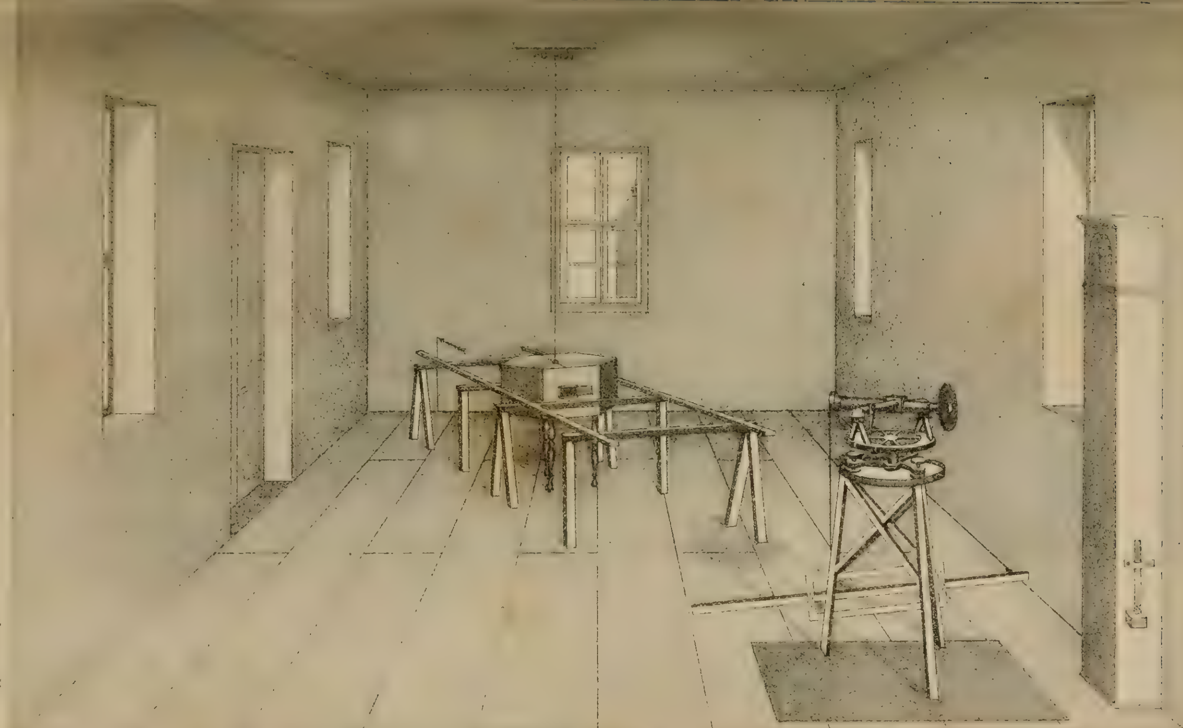
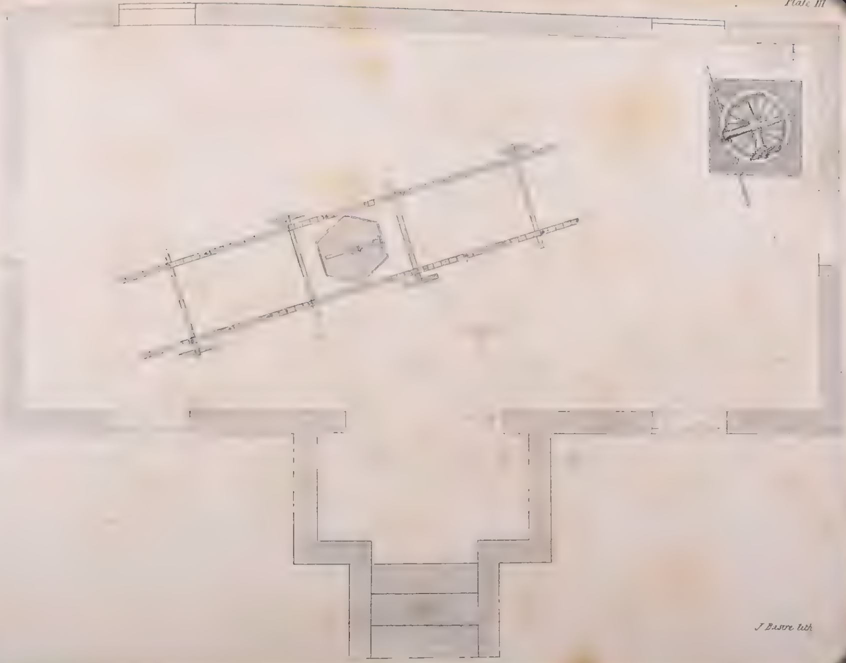


Magnetic Observatory, Göttingen



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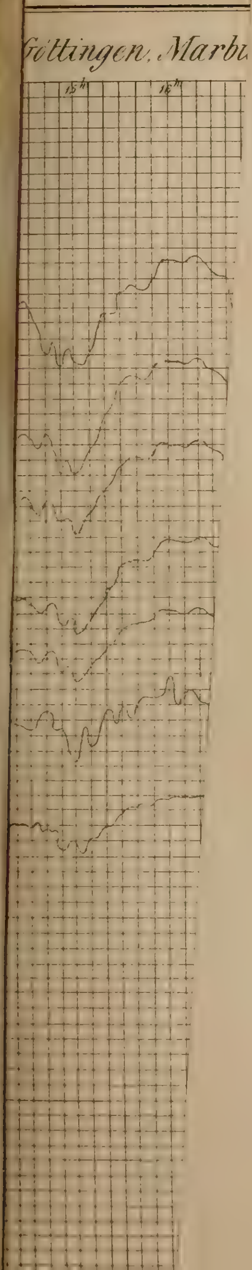
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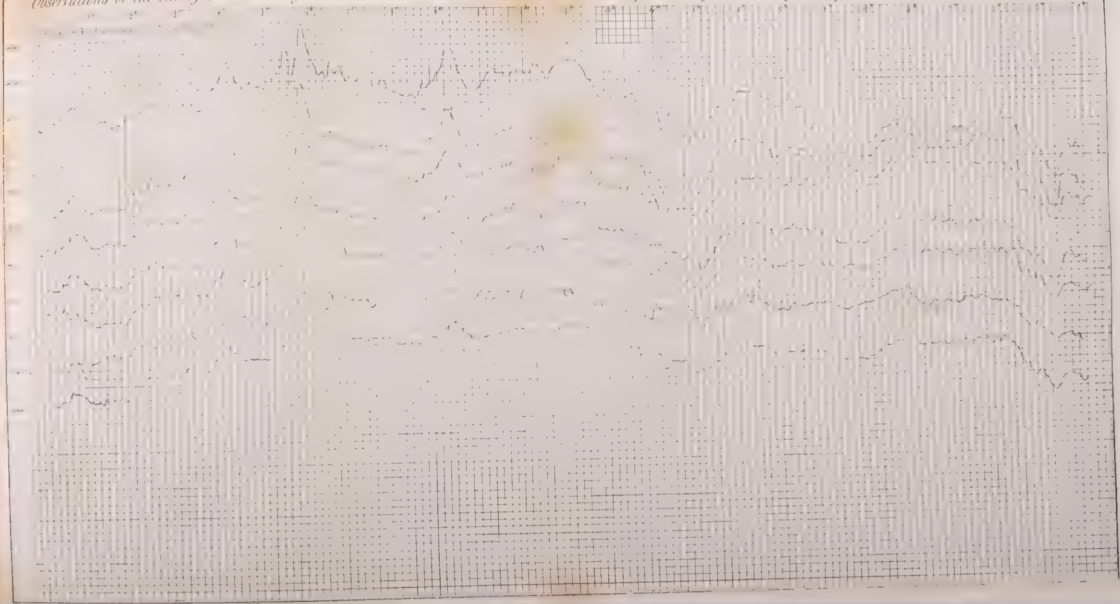
Observations of the changes of the Magnetic Declination, Nov.r 28th & 29th 1835, at the Hague, Göttingen, Marburg, Leipzig, Munich, Palermo, Milan &c.



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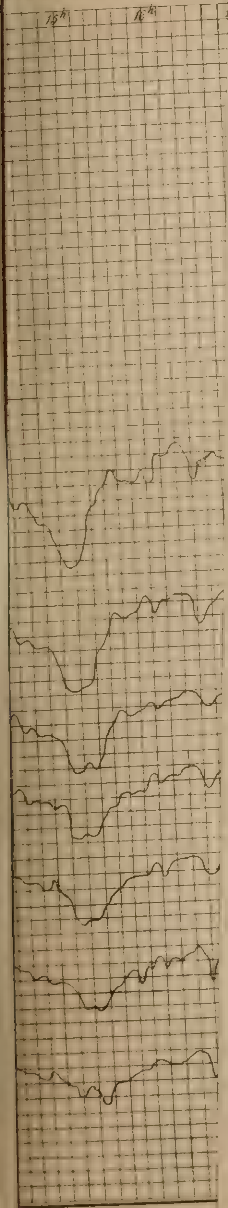
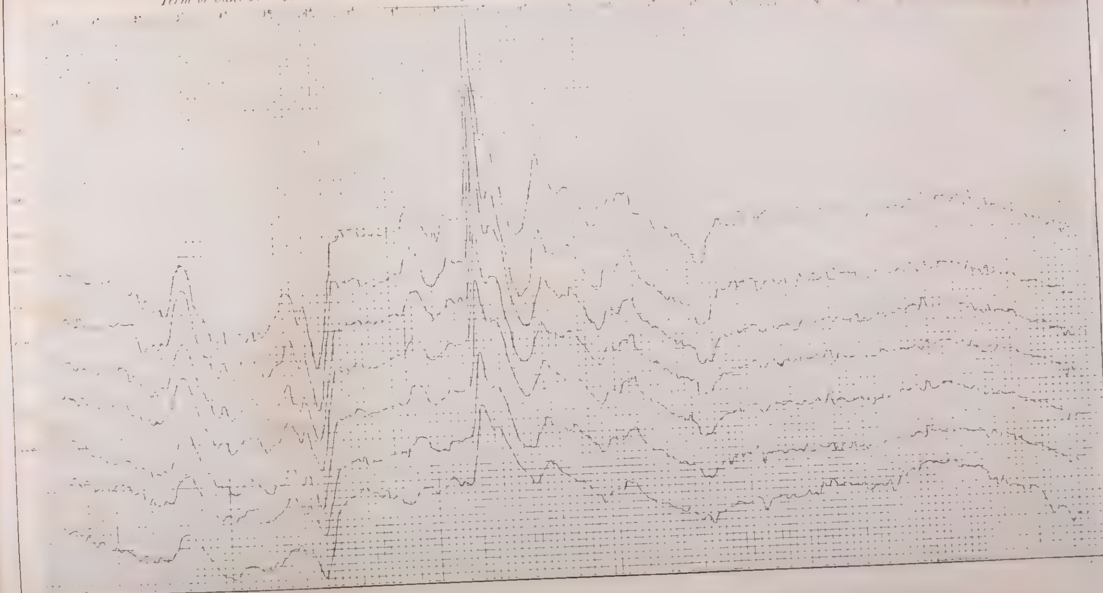


Term of Jan.y 30.th 1836. Observations at the Hague, Göttingen, Marburg, Leipzig, Munich, Milan, Catania.



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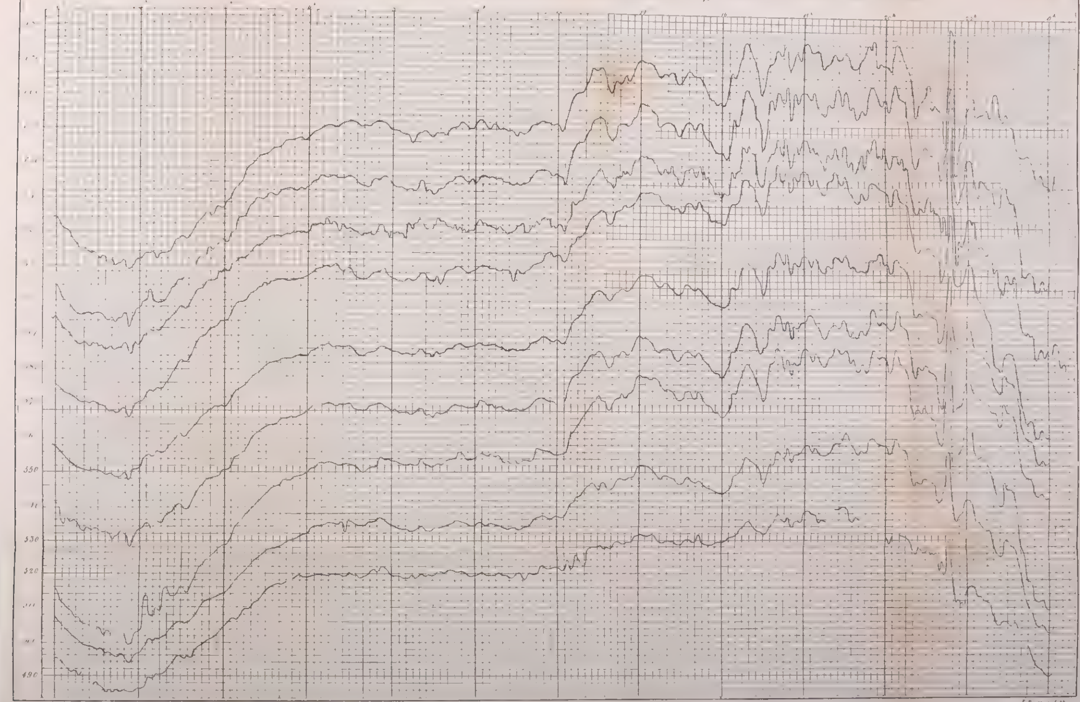
Term of July 30th 1836.

Observations at the Hague, Gottingen, Berlin, Breslau, Leipzig, Marburg, Munich, Milan, Messina



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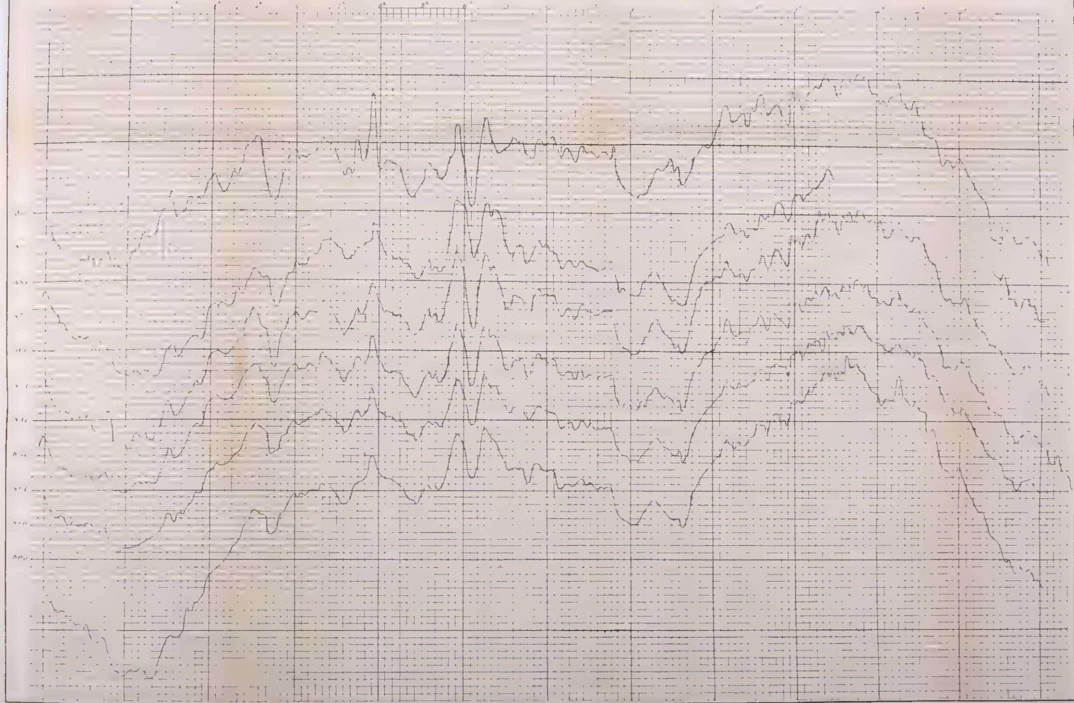
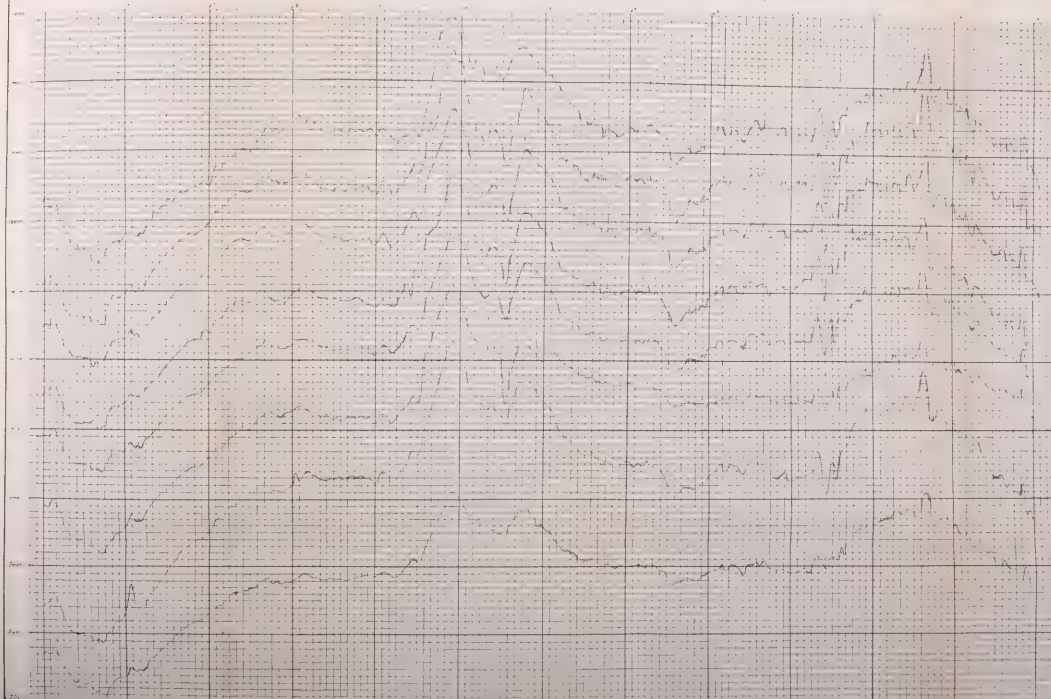
Term of August 17th 1836.

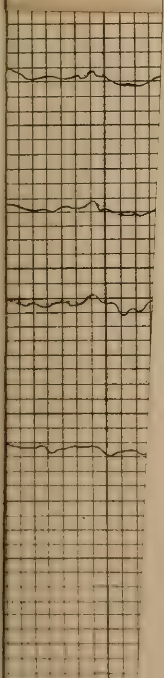
Observations at Upsala, the Hague, Göttingen, Berlin, Leipzig, Munich.



Term of Sept.^r 24^th 1836

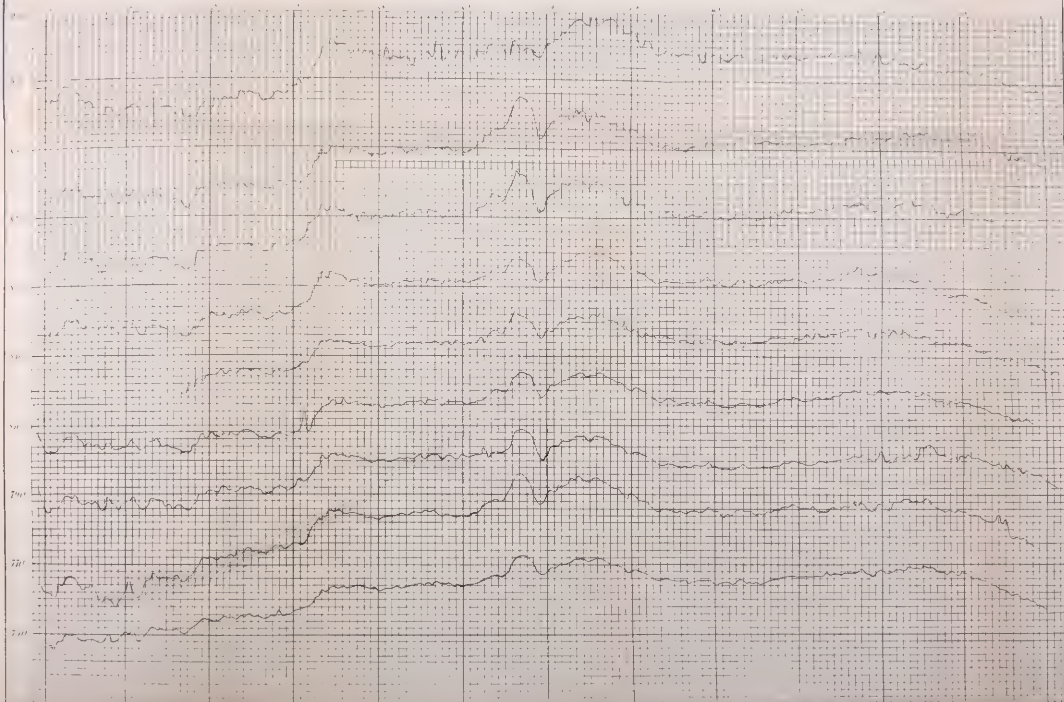
Observations at the Hague, Göttingen, Berlin, Breslau, Leipzig, Marburg, Munich, Milan.





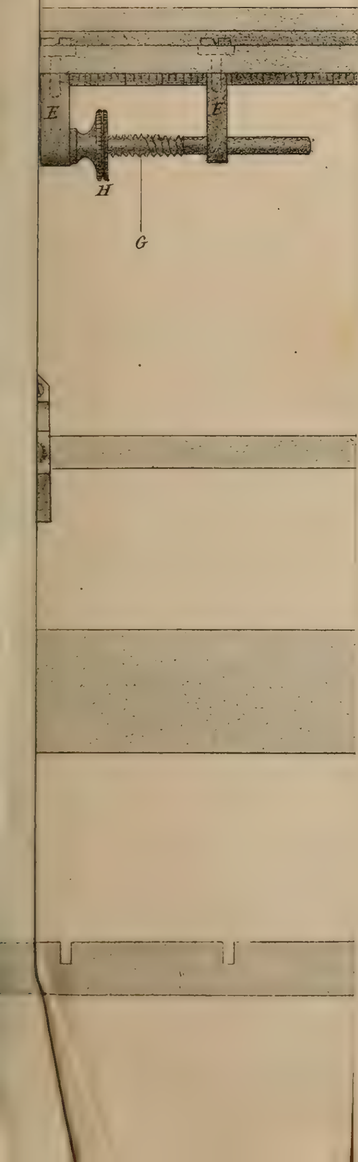
Term of Nov.r 26th 1836.

Observations at Upsala, Breda, Gottingen, Breslau, Freiberg, Leipzig, Marburg, Munich, Milan (



ic A

Fig. 2.



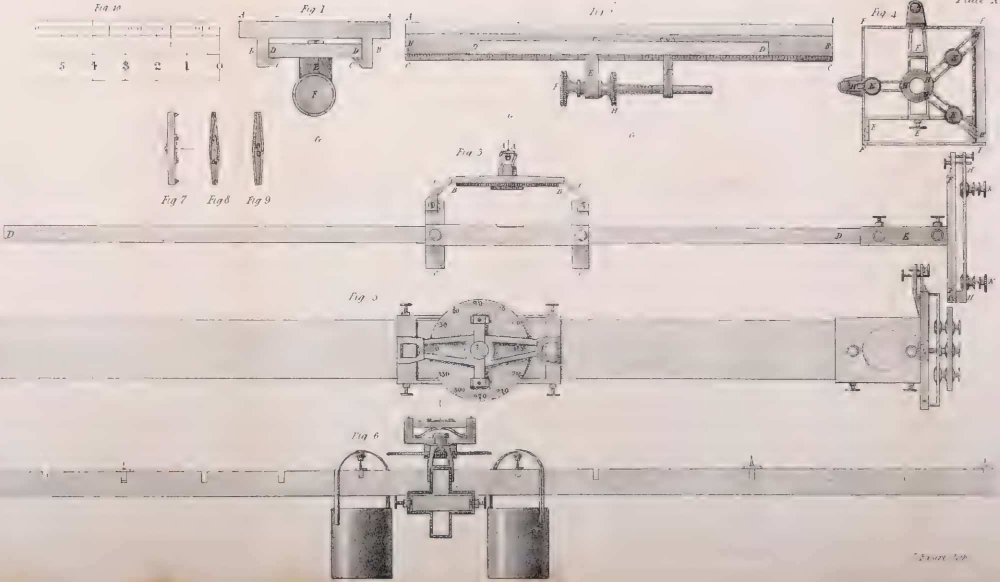


Fig 10
Fig 1
Fig 3
Fig 4
Fig 5
Fig 6
Fig 7
Fig 8
Fig 9

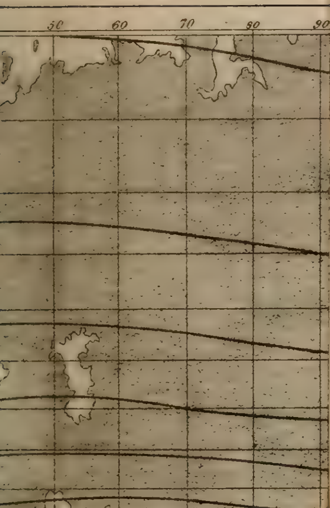

50
60
70
80
90

Chart for the Value of $\frac{V}{R}$.

1st Part

Pl. XI

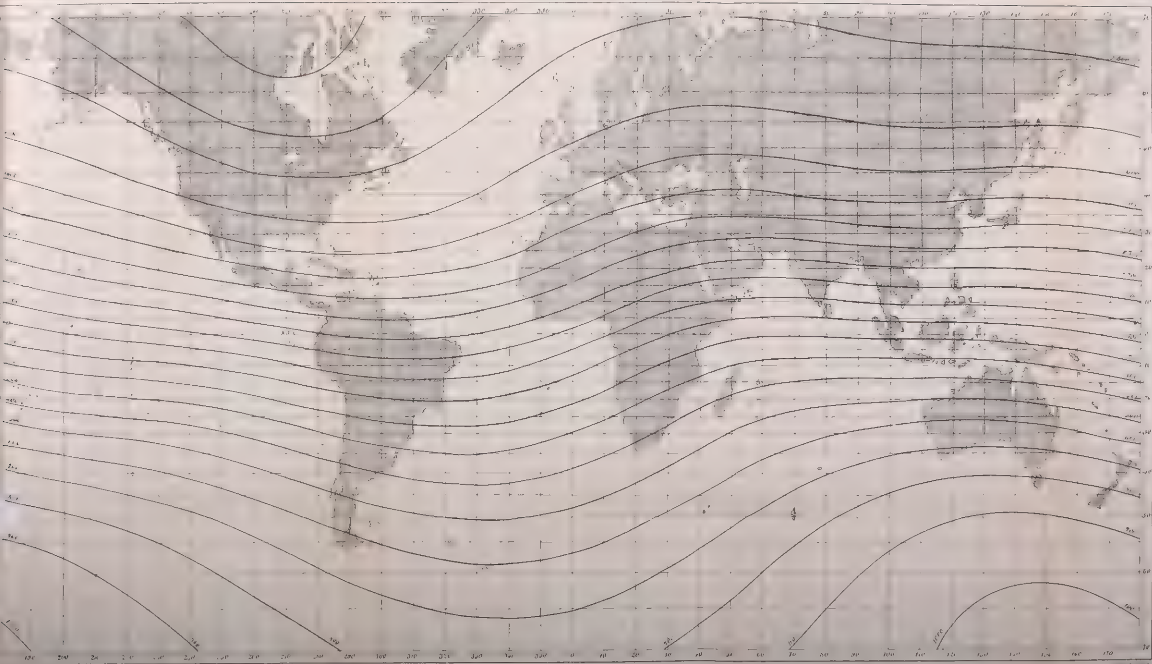
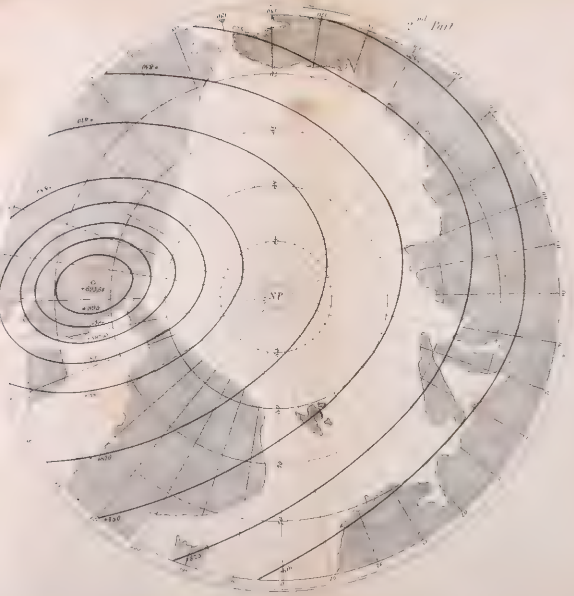
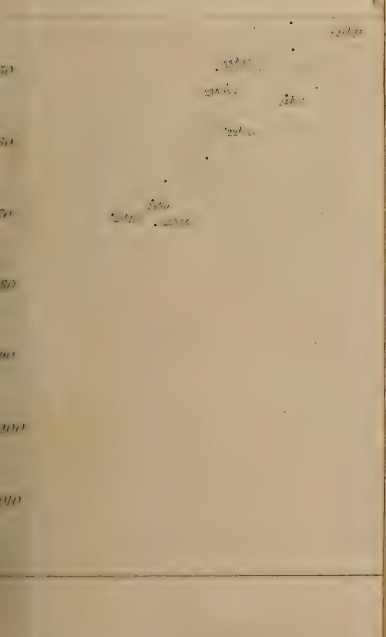
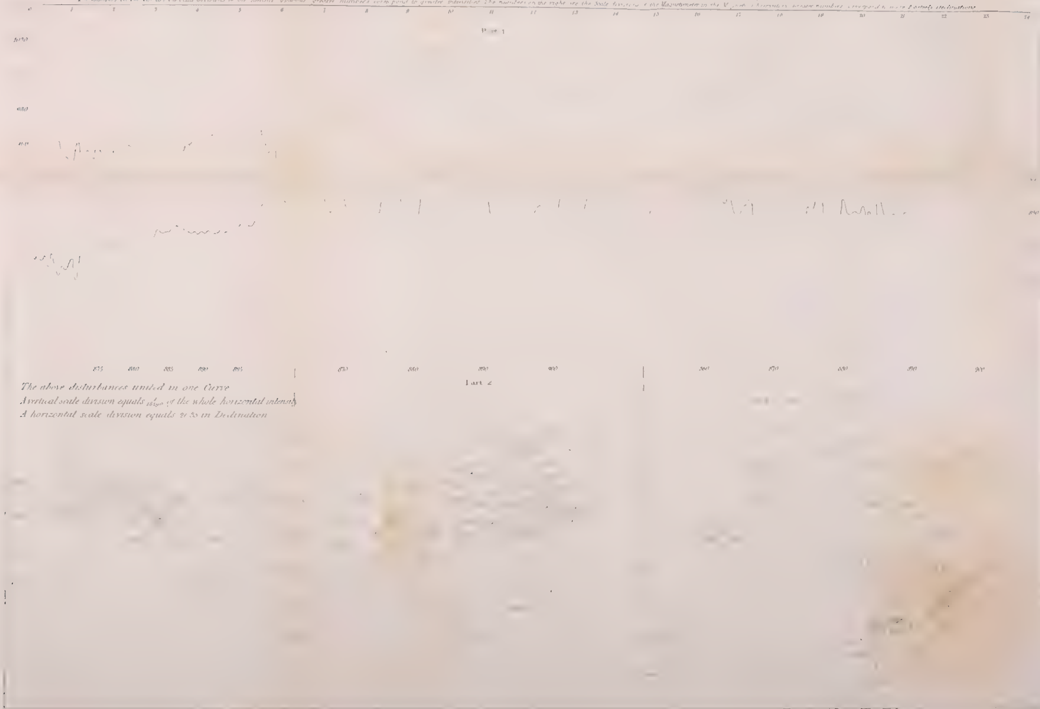


Chart for the Value of $\frac{V}{R}$.





Disturbances of the horizontal magnetic Intensity (upper curve), and of the magnetic Declination (lower Curve) Göttingen 1837 November 13, 14



The above disturbances united in one Curve
A vertical scale division equals [illegible] of the whole horizontal intensity
A horizontal scale division equals 21.35 in Declination

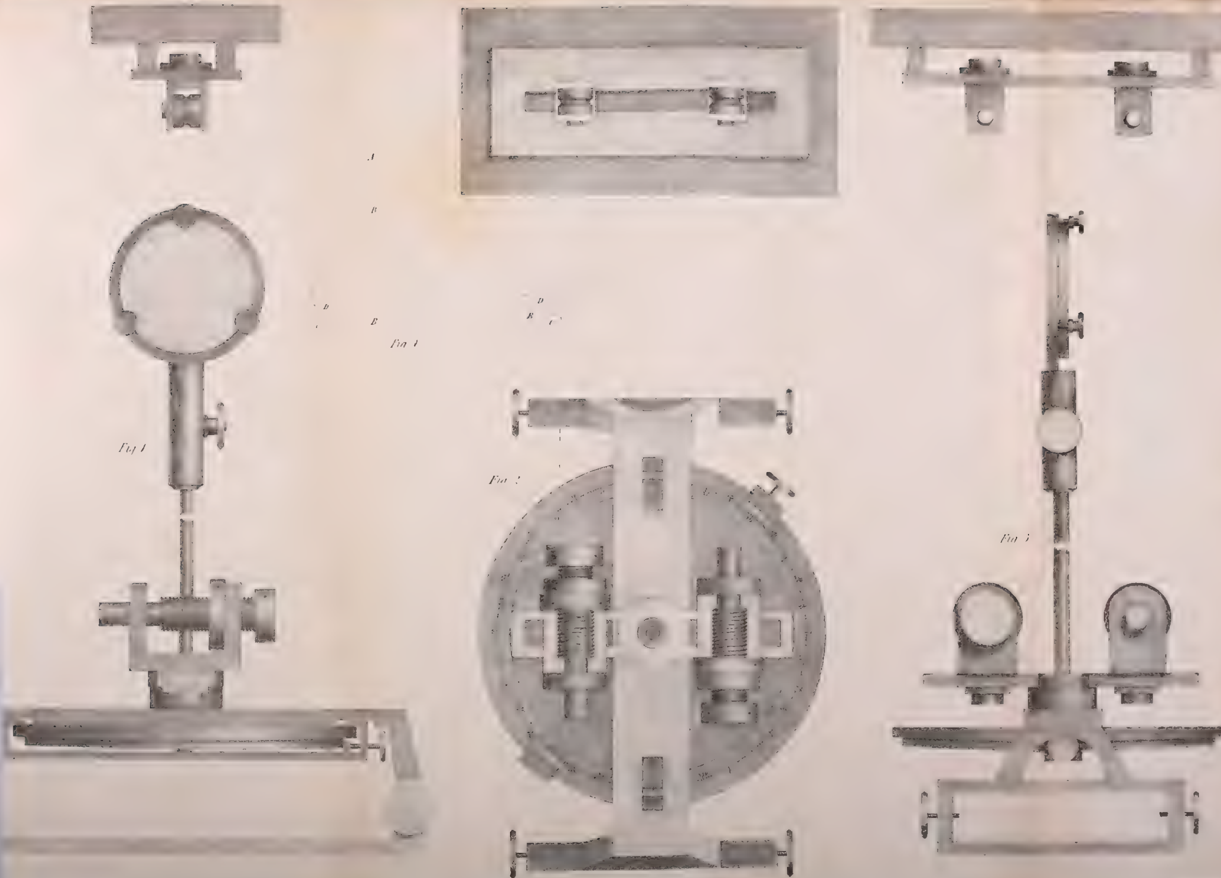
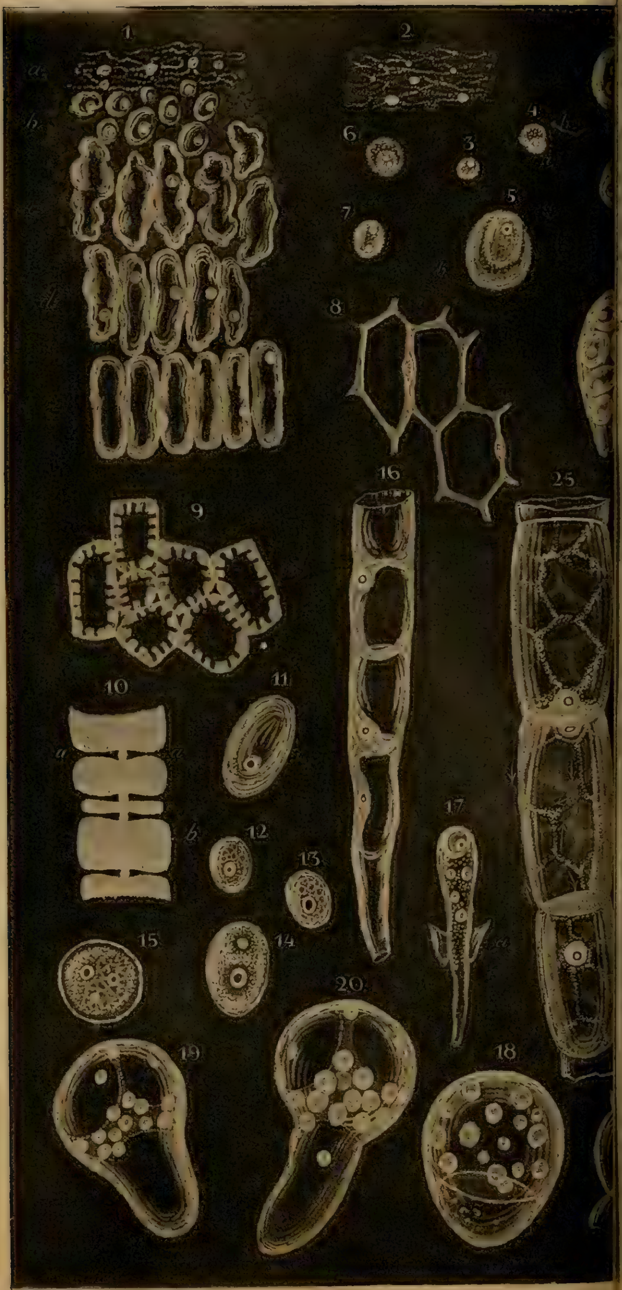
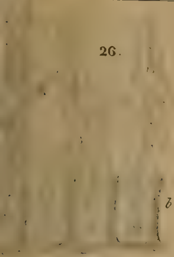


Fig 1
Fig 2
Fig 3
Fig 4
A
B
D
E
C



Dr. Schleiden. n.d. N. gezeichnet.

26.



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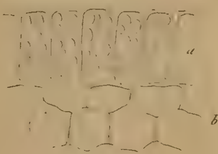
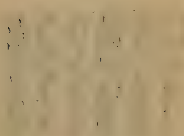


7.

32.



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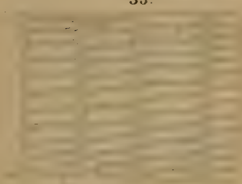
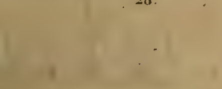


28.



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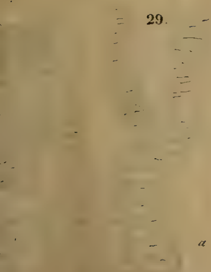

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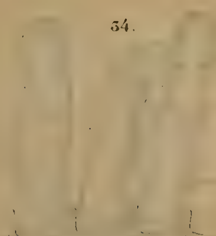
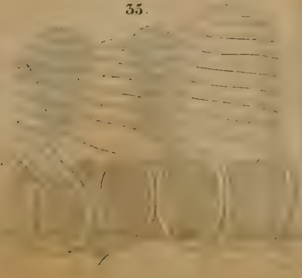
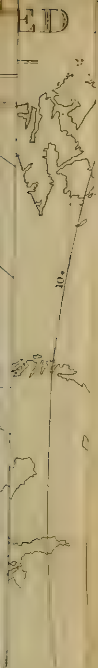


0.

35.



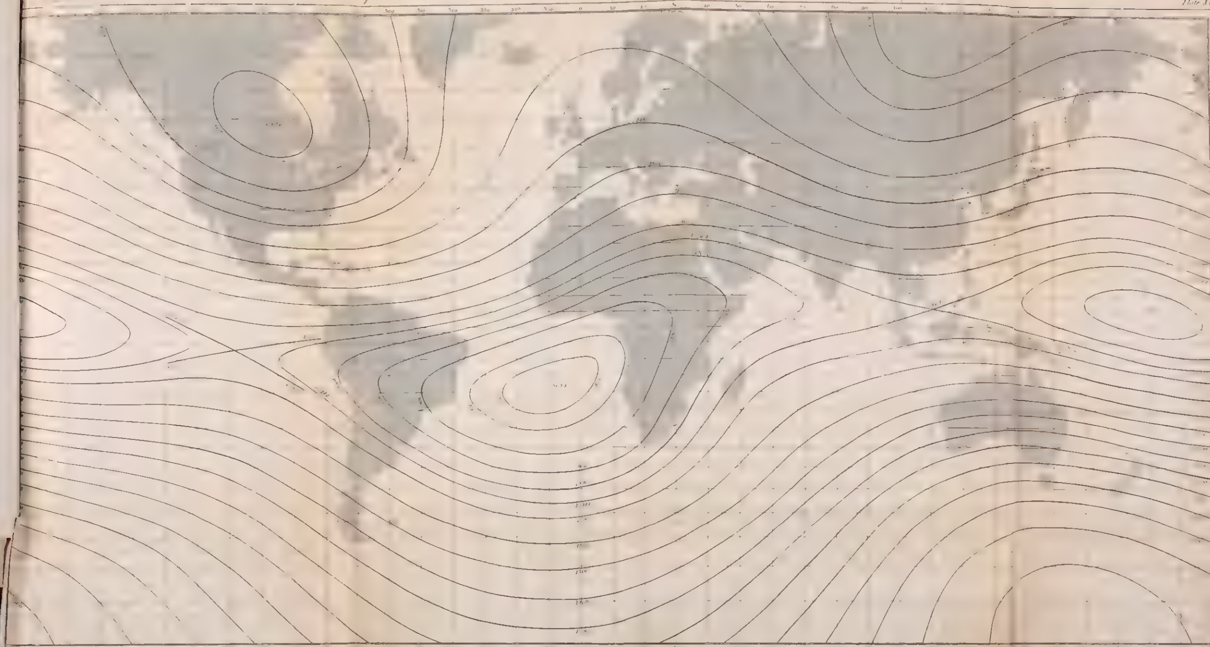

ED

THE LINES OF MAGNETIC DECLINATION COMPUTED ACCORDING TO THE THEORY OF M. [illegible]


40

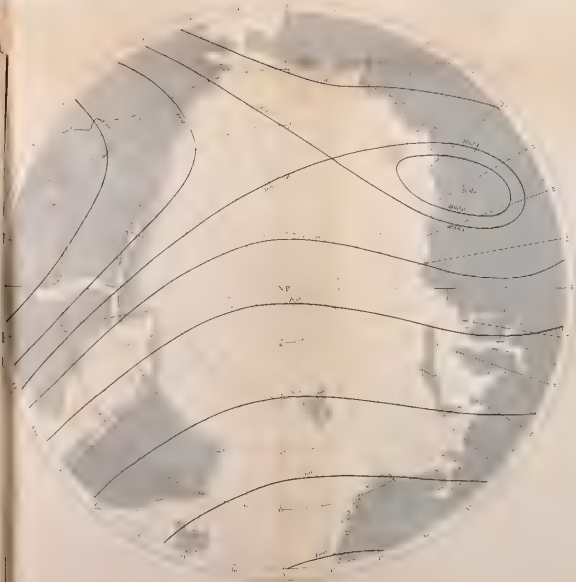
The lines of total Magnetic Intensity computed according to the theory of M. Gauss.

Plate XVII.

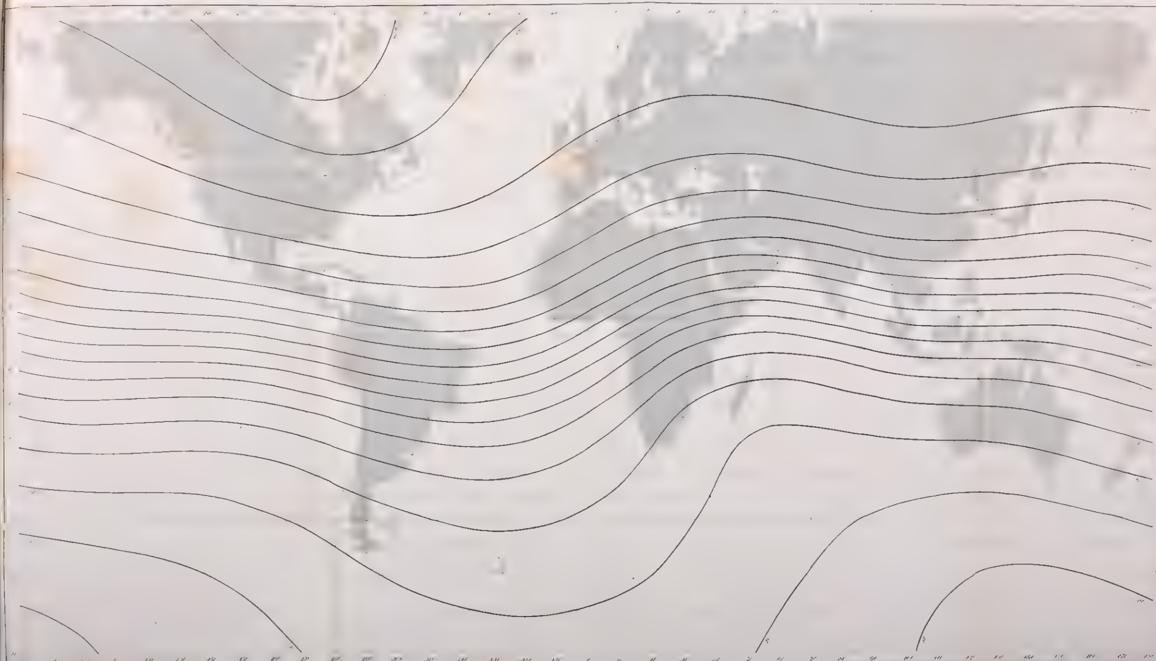


4.—The lines of total Magnetic Intensity computed according to the theory of M. Gauss.

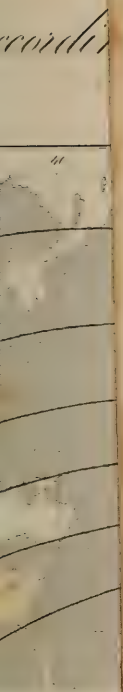
Plate XIV



Map of the lines of equal Inclination computed according to the theory of Mr Gauss.

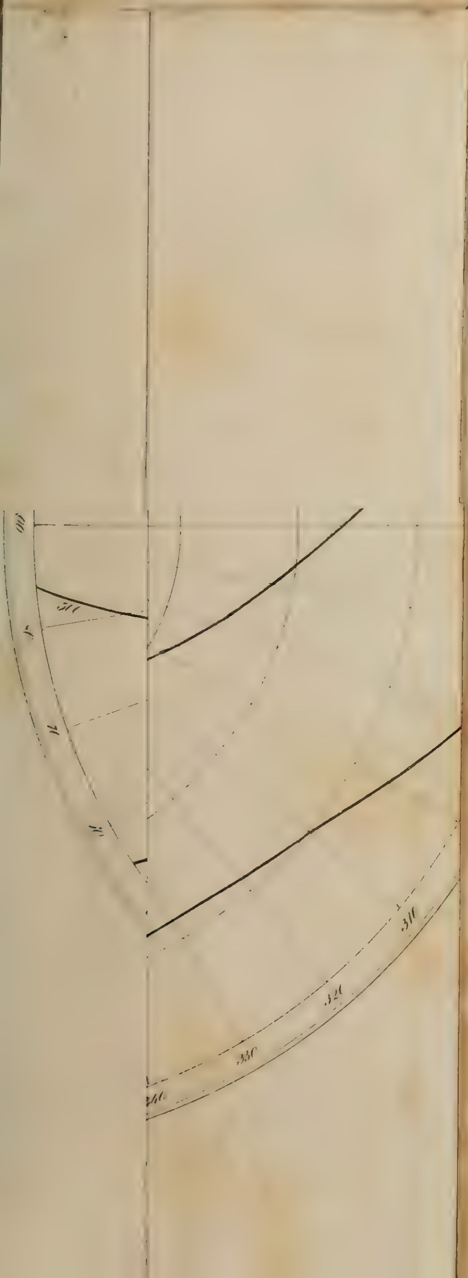


Map of the values of the Inclination computed according to the theory of M. Gauss

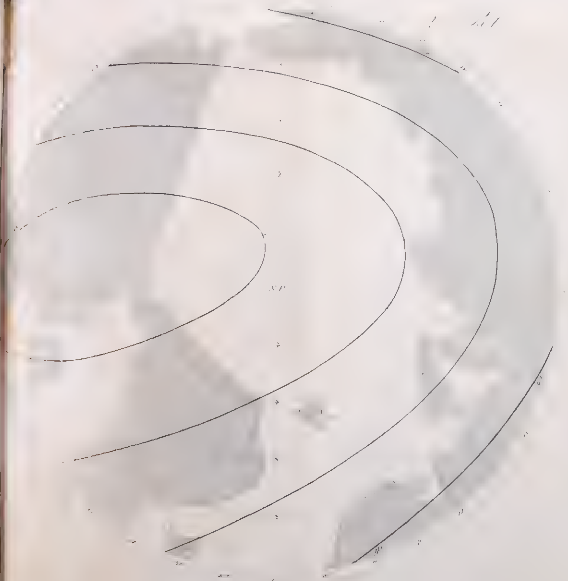


Map of the values of the horizontal Intensity computed according to the theory of M. Gauss

Pl. [illegible]


310
320
330

Maps of the curves of the horizontal Intensity computed according to the theory of Gauss.



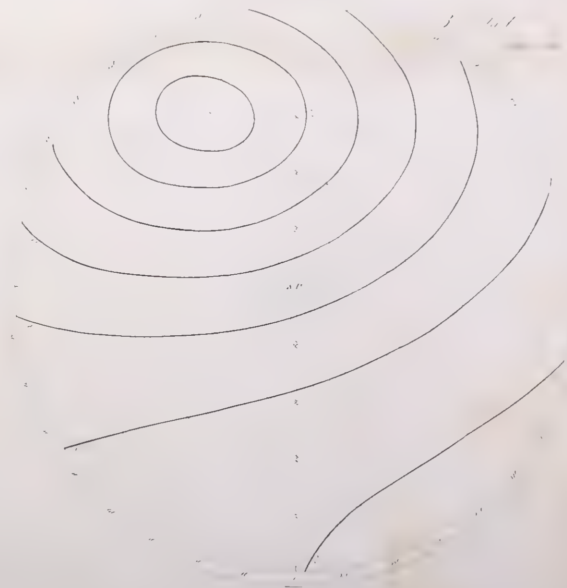


Fig. 1.

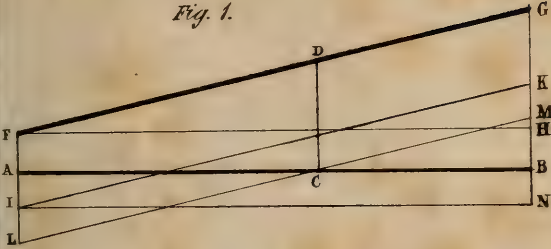


Fig. 2.

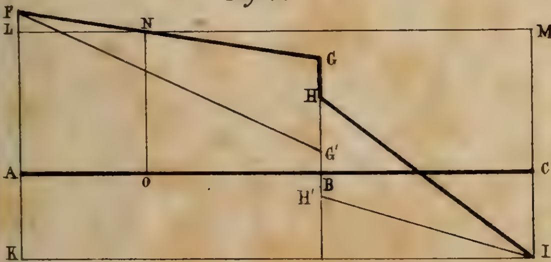
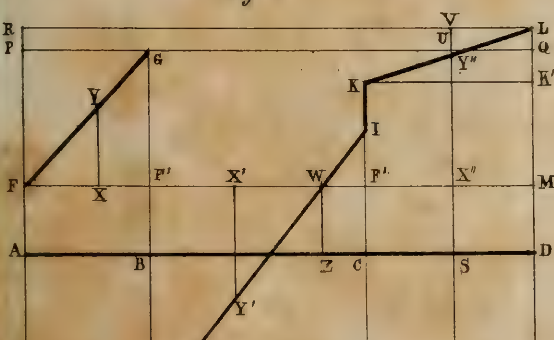


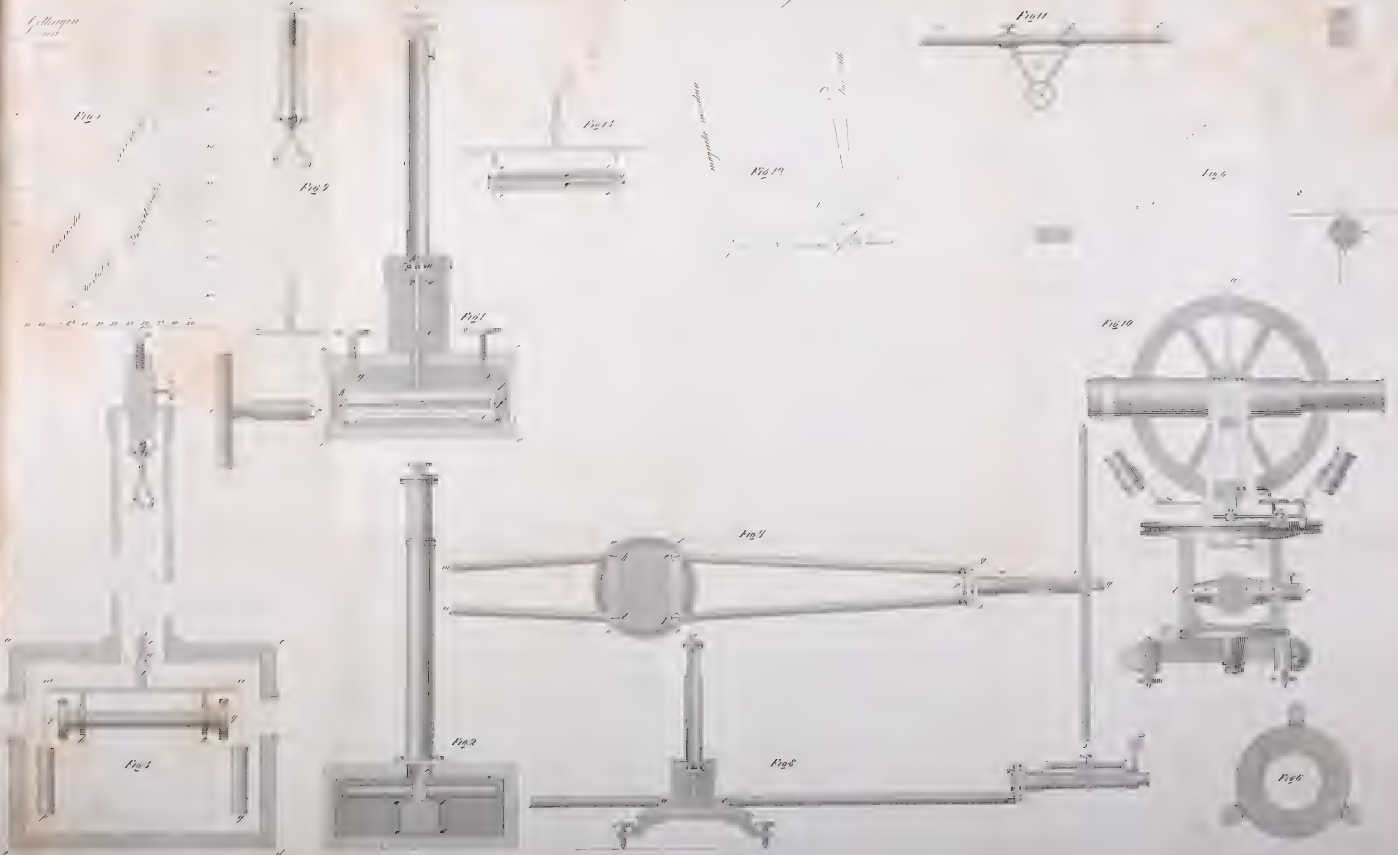
Fig. 3.

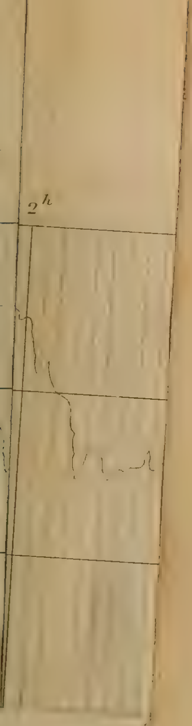


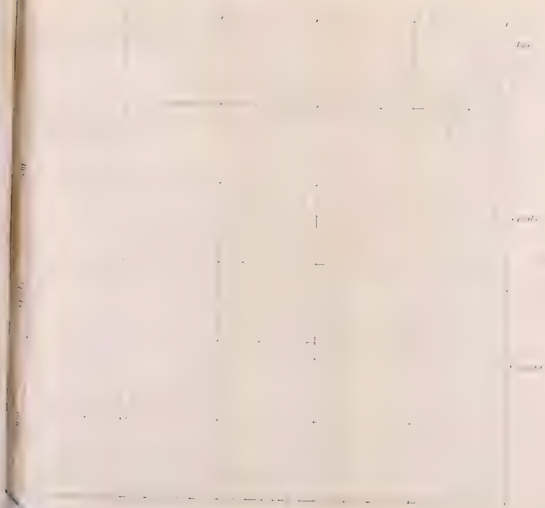
J. Basire. lith

Ohm on the Galvanic Circuit.

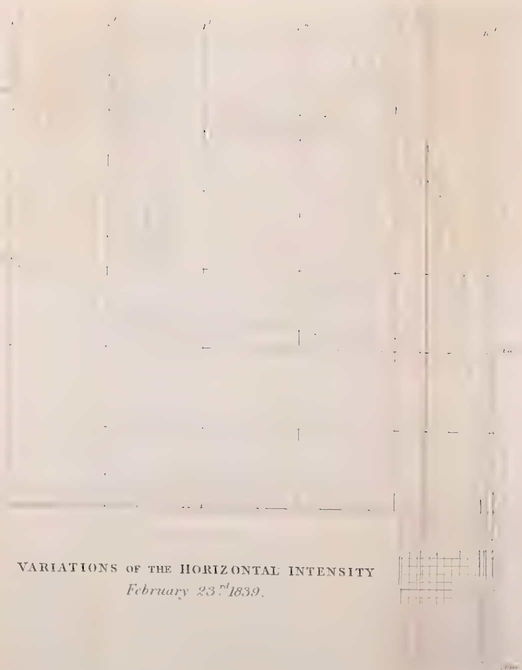
Weber's transportable Magnetometer




2^h



VARIATIONS OF THE MAGNETIC DECLINATION
February 23rd 1839.



VARIATIONS OF THE HORIZONTAL INTENSITY
February 23rd 1839.